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OF IGNORANCE:

REPLIES TO THE CRITICS OF HANDWRITING EXPERTISE

Second, Enlarged, Edition of
"Exorcism of Ignorance:
A Reply from a Handwriting Expert"

MARCEL B. MATLEY

San Francisco, CA
2000

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REPLIES TO THE MISCONCEPTIONS
AND MISUNDERSTANDINGS OF THE CRITICS
OF FORENSIC HANDWRITING EXPERTISE

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Dedicated
to all those who speak
out of misconception and misunderstanding
and thus provide others with
opportunity for demonstrating
knowledge, understanding and expertise
while maintaining both courtesy and patience,
curbing that all too human impulse both to propound
out of profound ignorance and
to exercise freedom to criticize uncritically.

"EXORCISM OF IGNORANCE:
A REPLY FROM A HANDWRITING EXPERT"
A LETTER TO THE EDITOR OF
UNIVERSITY OF PENNSYLVANIA LAW REVIEW

The following is the text of a letter sent to the editor of the UNIVERSITY OF PENNSYLVANIA LAW REVIEW in reply to the paper published in volume 137, pages 731-92, January 1989, *Exorcism of Ignorance as a Proxy for Rational Knowledge; the Lessons of Handwriting Identification Expertise*, authored by D. Michael Risinger, Mark P. Denbeaux and Michael J. Saks. In 1989 the Law Review sent a note declining to publish the reply. Whether it was a compliment or a rubbing of salt into a wound assumed sustained by the first rejection, in 1990 another note declining to publish the reply was received. I issued it as a monograph, made a direct mail notification of the publication to the profession of document examiners, and was gratified by its reception.

I owe a debt to the authors, because such a limited reply to just a few of their many errors in logic and fact helped build my reputation as a competent author in questioned documents. In gratitude, I dedicate this monograph to them and others who have made their own reputations by rejecting that which they do not understand and of which they know very little, and of what they do know much of that is incomplete and incorrect. They hold promise out to all of us that success does not demand that one bother with either accuracy or the hard work of proper learning. Just expound, propound and compound the errors, the more easily so the less one is weighed down with reality. There may well be many to reward one with renown for such easy authorship, if not with wealth.

The letter is reprinted as first issued, including format of bibliographic citations.

THE LETTER

August 6, 1989

Dear Editor:

Regarding the article, "EXORCISM OF IGNORANCE AS A PROXY FOR RATIONAL KNOWLEDGE..." (9), may I offer the following reply as a member of the Witchcraft Expertise.

First, there is the unstated argument: the literary attitude in the style of writing and choice of words. It is a smart-aleck and condescending dismissal through insult. As one matures one learns that superiority is not a matter of a superior attitude of that kind.

I would have liked to see the authors discuss their position in a more mature manner. Use of slightly veiled adjectives of insult may have its place in the courtroom defense of a weak case; it should not be tolerated in an academic periodical. I will now leave consideration of all ad hominem arguments aside.

The heart of the argument is that handwriting experts are fifty percent incompetent and avoid competence testing. Peripheral arguments point out that their testimony may lack substantive facts and scientific principles to support it and that the authors had difficulty in locating the specialized publications, which then did not have sufficient empirically based studies.

They discovered that Wigmore and Osborn were human with faults, and that their system of evidence was not perfect. The unstated conclusion apparently is that there ought not be any authentication and identification of handwriting in our courts of law. I will consider these points in reverse order.

Before the time of Osborn and Wigmore there was virtually no handwriting expertise in American Courts, indeed practically no expert testimony at all. The authors could have read Osborn's *Questioned Documents* (7). I believe they would have found ample court opinions putting down the handwriting expert and thus saved considerable time finding the ones they did use. Osborn's article in the University of Pennsylvania Law Review (5) gives ample opinions the other way, the new outlook which brought a halt to an easy avenue of success for willing forgers and perjurers.

Previously only writing in evidence in the case could be used as exemplars. The forger could simply supply more correlative documents, while his attorney objected to anything that could demonstrate the forgery. This situation died hard and slowly.

Keeler (2) told of an election dispute where ballots were marked while stacked. The X on one was embossed on the one beneath. The expert asked the judge to look at the original in sunlight by the window to see the obvious match between embossment of one ballot and mark on another. The judge disparaged the expertise of the expert and said that in the "excellent" courtroom light he saw no such evidence. Therefore he ruled that the obvious did not exist. Such was the attitude Wigmore and Osborn overcame.

As to Wigmore and Osborn being faulted humans and their proposed system of evidence having flaws, I am afraid that is true of all of us who are members of the human race. We and our institutions are perfectible. However, if that premise forces the conclusion that given persons and their contributions must be discarded or at least disparaged, the unavoidable conclusion is despair for us and for all our leaders and institutions.

At the end of the bibliography the authors sum up their evaluation of handwriting studies: "The vast majority of the handwriting analysis 'studies' in these publications

consist of anecdotal observations, hypothesis creation, and speculation" (9, p.784). Having seen virtually all the document examination and handwriting expertise articles in the periodicals listed, excepting THE CANADIAN SOCIETY OF FORENSIC SCIENCE JOURNAL, plus many not in those periodicals, I find that nowhere are anecdotal studies presented as in-depth research.

By and large these authors are most cautious in putting forth new hypotheses. If they do, they clearly present them as such, recognizing what would be needed for further study. Speculation is relative to objective data and its implications for practical document examination. Thus the quoted sentence is a misrepresentation by implication.

In our field we do not have government or foundation funds available for research. The research done is mostly at the time and expense of the ones doing it. Yet there are very substantive studies done, many appearing in the periodicals listed, for example that by Livingston (3).

They had difficulty in locating periodicals and concluded: "Because most forensic sciences have little academic base, these materials were hard to obtain" (9, p.784). I cannot believe Boston is so impoverished. Here in San Francisco I can show you all but three of them within a two block area and two others just across the Bay. In those and other journals within that two block area, I located about 300 relevant articles.

Then if one wishes to drive an hour or so, there are two State Universities with vastly more material. These institutions offer degrees in this allegedly non-academic field! As a matter of fact, document examination draws on a vast array of academically recognized sciences, handwriting expertise being the heart, not the whole body of the work.

Testimony of experts is often without the data and scientific principles needed to back up the expert's opinion. Consider this: Osborn (8) strenuously objected to just such testimony. It was mere opinion if offered without the "reasons and reasoning" behind it, to use his specific expression.

Further, Osborn proposed that qualified experts be appointed by the Courts and paid for by parties requesting such expertise (6). He wanted partisanship and advocacy completely withdrawn from the functions of the expert. Indeed, the Code of Ethics (1) states that the expert should maintain objectivity and avoid advocacy.

However, in an advocacy system, those defending the questionable and having enough funds can shop around until they find or create a group of experts who have opinion for hire. Absolutely anyone at all, no matter what the background, can say: "I am a handwriting expert." No one can say there is no legal right to do so.

///////

We have no public licensing and testing. Private groups assume the role of certifying experts, but at times they are seen to have unstated prejudices which precede objective certification. Again, anyone at all can claim to be a certifying organization.

The solution is not to judge the competent as one with the incompetent, but it is two-fold. First, to establish an objective, public regulation and testing for certain recognized levels of expertise.

Second, to require every expert witness to give observable data, principles of interpretation and logical reasoning which support the opinion offered. Failing that on direct examination, the court should be required to strike the expert testimony from the record. Then cross-examination could delve into any weaknesses in the argument of acceptable testimony.

As a result, the fact finder in our court system would be able to judge whether the opinion were reasonable and valid as applied to the case at hand. In handwriting expertise, there is absolutely no excuse for a witness to fail to give such testimony.

Make legal history. Next time an expert witness on the opposing side fails to give the factual data, scientific principles and logical reasoning supporting the opinion, move that the testimony be struck as mere, unfounded, non-expert opinion.

Incompetence and avoidance of competence testing is the essence of the entire article. I wrote a guide for attorneys (4) on how to examine a handwriting expert. Personal attack is often used, which shows the weakness of the attackers' case or personality. A good attorney will depose the expert ahead of time or cross-examine in a systematic but simple and thorough procedure.

If you want a fair test of the witness' competence, any of us who know our stuff can devise one, of the kind that we ourselves would not shrink from taking. However, ultimately attorneys need to devise a way to know when a given expert is selling opinions rather than expertise. "You want this to be authentic? Well, this comma proves it. You want this other to be a forgery? Who do you want to pin it on?" Not that literally, but implicitly promised.

If you can always rely on any given expert in any field to back up any client on any case, you are buying opinions not expertise. You become the problem.

In the last analysis, expert testimony will be as expert and objective and honest as the legal profession is willing to have it be. Attorneys have a legal obligation to represent a client's legitimate interests; they have no ethical or moral justification for pulling off a client's skulduggery. Whatever one group is willing to buy, another group will be willing to sell it.

Even without legislation, Bar Associations can say what they believe constitutes various levels of expertise. Criteria can be set which are not subject to sleight of hand

and allow for the many ways people can achieve quality, knowledge and skill in a field. It is just a lot easier to look at paper qualification: this membership (had for so much a year), this certification (given to our buddies only), etc.

Most of my reports enable the attorney to settle the case before trial. So the number of court testimonies may really indicate incompetent or unconvincing reports. In the last analysis any expertise is proven by performance.

What proves one an expert trial attorney? Not passing the bar; that just makes one legitimate. It is excellent performance in the court room. So we need a valid, fair test of excellence. That is hard to devise, a lot harder than merely taking a haughty or disparaging attitude.

Sincerely,
Marcel B. Matley

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Ms. Bonjour described the procedures that she, and other experts in the field of handwriting analysis, employ as follows: First, the expert determines whether a questioned document contains a sufficient amount of writing and enough individual characteristics to permit identification. After determining that the questioned document is identifiable, the expert examines the submitted handwriting specimens in the same mariner. If both the questioned document and the specimens contain sufficient identifiable characteristics, then the expert compares those characteristics, e.g., the slant of the writing, the shapes of the letters, the letter connections, the height of letters, the spacing between letters, the spacing between words, the "i" dots and "t" crosses, etc. App. 136. After making these comparisons, the expert weighs the evidence, considering both the similarities and differences in the handwriting and determines whether or not there is a match.

United States of America; Government of the Virgin Islands vs. Edwin Velasquez, 64 F2 844, 33 V. Is. 265, at 269, footnote 3 (3rd Cir, 1995)

NONSENSE ABOUT
SCIENCE AND NONSCIENCE

In a follow-up paper to their article in UNIVERSITY OF PENNSYLVANIA LAW REVIEW, two of the three authors proceed to explain their spin on *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 113 S Ct 2786 (1993), as applicable to forensic handwriting expertise. One must credit the authors with persistence. It is hard enough to be wrong once about so many things, but to be consistently and insistently wrong about the same and even more things, all in the same mistaken direction, takes persistence in one's chosen direction and dedication and devotion to one's opinions. It is an example to those who pursue true science and think of forensic expertise first as a vocation and secondly as a business. Only the same persistence and dedication to providing scientifically, legally and technically sound expertise will eventually win the day over ill advised criticism. Albert S. Osborn and his colleagues accomplished it once; we can accomplish it again.

In 82 IOWA LAW REVIEW, 21-74, October 1996, appeared a paper by D. Michael Risinger and Michael J. Saks: *Science and Nonscience in the Courts: Daubert Meets Handwriting Identification Expertise*. They open with: "Document examiners do many things." After that they make mistakes in fact as well as in law. The article is 54 pages, and I am sure that the reader does not wish to plow through minutiae of every mistake the authors made following that unimpeachable opening. I therefor restrict myself to only those things of most importance. Others have provided replies to other aspects of the authors' opinions; those critiques are cited in the bibliography.

At page 22 the authors summarize the history of handwriting expertise. They quote Aristotle as quoted in a graphology book, which made no pretenses at being about forensic handwriting identification. As professors at prestigious universities, why did it not occur to them either personally or vicariously through their students, as professors are wont, to track down the original quote in Aristotle's own works? Scholars do not build on unverified rumor, no matter how authoritative or enticing. What makes their pretensions at scholarship more transparent is that they cite Camillo Baldi's work as an early attempt at a system of handwriting expertise, when he never mentions the matter. See Backman (1994). Why not cite ancient Roman authors on the topic, known through medieval and into modern times? Packard (1959) opens his paper: "That the comparison and identification of handwriting in courts of law is no modern aspect of forensic science is borne out in many instances by Roman literature and records. Marcus Cicero in the second *Philippic* accuses Anthony of an unhealthy knowledge of the subject; Quintillian in his *Institutio Oratorio* gives some very definite advice to lawyers concerning the examination of documents to be presented at court; and the 5th century legislation had

been enacted with regard to document examiners and their legal duties and qualifications (Code of Justinian, Ord. 49, title IV, chap. 11)." If the authors had surveyed document examination literature half as well as they claim, they would have learned about some of these things.

Their account of history becomes even more unhistorical. At pages 22-23 they assert of handwriting expertise in the eighteenth century: "However, no such claimed expertise then existed in the English speaking world." Then, citing cases where it was admissible, they say on page 24: "[I]t was not until an 1854 statute was construed to authorize such testimony that handwriting identification expertise became admissible in English courts." Did no one proofread the text before publication? If authors contradict themselves, it ought to be in widely separated passages, not within the same segment. They use the word "construed" regarding the interpretation of a statute which explicitly stated that the expertise would be admissible. The authors often choose words which imply that their evidence offers support for their theses which it does not. See Packard (1959) for a portion of the exact text permitting handwriting comparison evidence in civil cases. Handwriting opinion by one familiar with a purported writer's script was virtually always permitted even in criminal courts.

They are wrong about handwriting expertise not existing in England. It was always admissible in ecclesiastical courts of England. As an example of civil practice, in the case *Folkes v. Chadd*, 3 Doug 157, 99 ER 589 (1782), affirmed 3 Doug K.B. 340, 99 ER 686 (1783), it is stated at 99 ER 590: "I cannot believe that where the question is whether a defect arises from a natural or artificial cause, the opinions of men of science are not to be received. Handwriting is proved everyday by opinion; and for false evidence on such questions a man may be indicted for perjury. Many nice questions may arise as to forgery, and as to the impressions of seals; whether the impression was made from the seal itself, or from an impression in wax. In such cases I cannot say that the opinion of seal-makers is not to be taken."

In the previous century handwriting identification evidence was received in *Rex vs. Sidney*, 9 How. St. Tr. 861 (1683). Colonel Algeron Sidney was convicted of treason and beheaded on witnesses saying a handwritten book was by him. Five years later an appeal was successful on grounds that the book had been written 20 years before the trial and that identifying traits could change. The appeal decision said that the book had never been proved to have been written by him. The conviction was reversed, and, as they could not reverse the beheading, the case instigated the move to disallow such expert testimony in criminal trials. The original witnesses, however, had not been experts, but experts bore the brunt of the error. It was Lord Kenyon, whom the authors discuss

inadequately, who hopelessly muddied the evidential waters in regards to civil cases, creating the common law rule which the statute of 1854 corrected.

The next segment reviews admissibility of handwriting expertise in American courts. There are far better surveys on this matter, for example, Wigmore (1896).

Their review of admissibility in American courts settles into a subjective evaluation of Albert S. Osborn, an evaluation laden with emotionally packed words intimating that he was a talented and successful scam. However, they do not come right out and say the man was a scam. After saying Osborn was "clearly a man of exceptional intelligence and critical abilities," they say that he turned handwriting expertise from ugly duckling into swan, that he had a blind spot and "a kind of mystical faith...," and that he conducted with Wigmore "a public relations campaign on behalf of 'scientific' handwriting identification expertise." Note that they put "scientific" in quote marks. One hopes the authors never give one a compliment, since they pile snide insult on snide insult after a single glowing compliment to Osborn. They fail to mention how Osborn used motion picture film to study many people writing and built his theories upon such objective studies as well as upon practical experience. Or did they inadvertently fail to notice that fact?

The authors then review *Daubert*. Whereas their spin on *Daubert* is that every expertise must rigidly satisfy the several criteria given in that case, the later Supreme Court decision in *Kumho Tire Co. vs. Carmichael*, 131 F3 1433, reversed, No. 97-1709 (US Supreme Court, 1999), explicitly states how that interpretation of *Daubert* is wrong. After firmly establishing the rule that all expert evidence must be proven reliable and that there is no sharp demarcation line between scientific, technical or skilled expertise, the decision concludes: "And the [trial] court ultimately based its decision upon Carlson's failure to satisfy either *Daubert*'s factors *or any other* set of reasonable reliability criteria. In light of the record as developed by the parties, that conclusion was within the District Court's lawful discretion.

"In sum, Rule 702 grants the district judge the discretionary authority, reviewable for its abuse, to determine reliability in light of the particular facts and circumstances of the particular case. The District Court did not abuse its discretionary authority in this case. Hence, the judgment of the Court of Appeals is *Reversed*." [Emphases in original.] The Supreme Court intended that all expert evidence must be proven reliable in order to be admissible, by meeting those criteria, or any other reasonable set of criteria. The problem with such a sensible, explanatory ruling from the Supreme Court is that being reasonable is a lot harder work than being dogmatic and flippant.

At page 30 they address the *Starzecpyzel* case. Since I will discuss that case in a later section and indirectly refute the incorrect spin the authors put on it, I make just one

observation here. In footnote 69 on page 31 they speak of a portion of the *Starzecpyzel* decision which is not to their liking. They describe it as "ambiguous and unclear." Anything the authors find which goes against their thesis, but which they cannot get around, is characterized as "unclear" or "ambiguous." For example, on page 32 they say the decision in *United States vs. Velasquez*, 33 V Is 265, 64 F2 844 (3 Cir 1995), was "more ambiguous" than *Starzecpyzel.*

The *Velasquez* court states at 33 V Is 268: "Following voir dire examination on the admissibility of Ms. Bonjour's testimony, the trial court rejected the defense's arguments that handwriting analysis did not constitute a valid field of scientific expertise." The Court of Appeals affirmed the trial court's rulings in Ms. Bonjour's regard. How more unambiguous can trial and appeal court rulings be? If you were to read their discussion of *Velasquez* without knowing which case it was, and then read the case report itself, you would never realize that it was one and the same. The authors drive home one lesson for all of us: Never accept anyone's interpretation of case law without first reading the relevant case yourself in its entirety. At page 279, footnote 9, the *Velasquez* court shares an even more unambiguous opinion, one by Ms. Bonjour: "Ms. Bonjour acknowledged that she had read Professor Denbeaux's law review article, although her critique—'it's a lot of gibberish'—was less than glowing. App. 164."

At page 34 they begin a discussion titled: "Future Directions in the Law." Do not bother reading their prognostications, since reality has subsequently triumphed in the courtroom, at least in *United States vs. Paul*, D.C. Docket No. 1:97-CR-115-1-GET, appeal from N.D. GA, No. 97-9302 (11 Cir 1999). It was their former co-author, Mark P. Denbeaux, not the government's expert, who was ruled inadmissible by the trial judge, a ruling upheld upon appeal. The government's handwriting expert at that trial happily combined within himself the two most opposed camps within the forensic handwriting identification community. An earlier curriculum vitae of his candidly noted that he taught the eight basic steps course of Graphoanalysis, while he is currently certified by American Board of Forensic Document Examiners.

On page 36 begins a segment titled: "The Possibility of a Science of Handwriting Identification." In summary, their theory of identification makes it impossible to identify anything. They say: "Thus, as science has known at least since Hume, for any identification whatsoever there is some residual probability of error." Can you imagine what transpires in their lives, if that is true or if, not being true, they believe it to be? One of the authors comes home after work and muses: "It seems to be my home, but I can't really identify it for sure." Anyway, he yells: "Honey, I'm home. If you are my honey I am home if this is my home." She, being a dutiful wife who is in harmony with her purported husband's alleged theories of identification (remember, she also can at best only

come up against a residual probability of error in all of these things) responds: "Let me see your driver's license. It resembles you. However, the all but residual probability of error in the license multiplied by the all but residual probability of error in your person multiplied by the all but residual probability of error in this house being your home multiplied by the same that it is my home multiplied by the same that it is the home it was yesterday and multiplied by all the other all but residual probabilities that either of us got any element of identification correct amounts to an ever enlarging factor of much more than residual probability this is all wrong." The authors say that "it would be nearly deranged to worry about the residual probability of error." Which may be their excuse for not worrying that they have even the most microscopic concept incorrect. After all, it is only residual scholarship at best. Why worry about it?

The reader ought not be dismayed that the authors believe the theory that a residual probability of error is the best science can do. In footnote 138 on page 57 they assert that, in proving expertise, "The important thing is how often one makes a judgment which is wholly correct when given the opportunity." In a word, a residual probability of error will not do, at least for those they disapprove of.

In all seriousness, the authors begin their theory of identification with a concept of "tagged residue." It is a muddled concept as they explain it. They actually maintain that the memory of an eyewitness is a tagged residue, and they claim that the concept is foreign to forensic expertise. Did they ever hear of identification of inks and other common substances by means of chemicals used as tags? Thornton (1978). If they heard of it, it seems not to have tagged their memories with any residual knowledge. The concept of tagged residue as the basis of all forensic identification is inapplicable to handwriting identification. Handwriting, as an act, is behavioral and cultural. A mechanistic mentality will never master that which is cultural and behavioral. Further, handwriting as a skill is an ever evolving thing. Because the authors apparently exhibit no understanding of this, their criticism is not of the reality of forensic handwriting identification, but of the unreality of their own misconceptions.

On page 38, they make a wondrous statement: "There has been no theoretical revision [of forensic handwriting identification] of any significance in nearly a century, and there is no professional encouragement or reward for attempts to falsify those theories that exist." Their assumption seems to be that any unrevised theory more or less a century old is to be trashed if not distrusted. The American Constitution and its Bill of Rights are more than two centuries old. Vast portions have never been amended, that is, revised. Our Declaration of Independence is a bit older, and its theory of our independence is unrevised to this day. The scientific method which states we should gather data empirically and consider revising our hypotheses concerning our data if and

when more or better data becomes available, is more than 23 centuries old. Yet the authors work off that method, however much they warp it. By their own stance they should abandon it since it has hardly suffered 23 or more significant revisions, which would have cumulatively altered it beyond recognition. As a matter of fact, the core theory and method of handwriting identification established by the classical authors in the field are so superbly designed that some handwriting experts are in trouble precisely because they have failed to master and maintain their pristine implementation.

The second concept in the quote, that falsification is a keynote of science, is itself neither falsifiable nor verifiable. If falsifiability is the key empirical proof in science, then science has nothing to offer in support of forensic identification. Identification is accomplished through verification. Falsification verifies nothing, so it leaves one in ignorance of what possibly might be, giving only a skilled surmise at best.

The most positive contribution the authors offer is to feather their own academic bed. On page 38 they claim: "Moreover, although is it not a logical *sine qua non* of science, there is virtually universal recognition that academic institutions will play a significant role in the practice of science and the training of scientists in nearly every area." Who would say that Archimedes, Burbank, Pasteur, and many more who were major contributors to scientific knowledge throughout history and worked outside of academic institutions, were any the less scientific? Thomas Edison and the heirs of his system of laboratory investigation did not labor in academic institutions employing the likes of the authors to propound on what is and is not science. I had a teacher who used to remark when she considered some people's attitudes: "My mind is made up. Do not confuse me with facts."

Beginning at page 39 is the segment: "The Claimed Principles of Handwriting Identification." The authors uncritically accept the two given by the government's *Daubert* hearing expert in the *Starzecpyzel* case: "No two people write exactly alike," and "No one person writes the same word exactly the same way twice." It takes no more than a most minimal capacity for logic to see that those two are at best principles of non-identification. They only tell us that any two writings, whether by the same or two different persons, are different, which we knew already. Only a principle of similarity, not a principle of difference, can serve as the basis for a theory of identification, since identification must be based on some kind of similarity.

Handwriting identification is based on the fact that twelve factors combine to make any person's writing individually unique (Saudek 1929, 1976). What the expert must do in any specific case is establish which of these twelve factors actually operate to make the questioned writing on the one hand and the exemplar writing on the other uniquely individual enough to be identified as by its author and no other, this being done

for each writing in and of itself prior to any comparison between them. The amount of uniquely individual features required for identification will depend on the potential population of writers who are reasonable suspects in the given case. The best way to make absolutely certain of what individualizes a suspect's writing within the context of a given case is to make an equal examination of writings of all reasonable suspects.

At page 41 the authors assert that they had made a full literature search prior to their 1989 piece. Professor Risinger purchased three copies of my monograph which had the letter to the editor which is reproduced as the first chapter of this monograph. I sent gratis a copy of the first edition of my *Health and Handwriting, an Annotated Bibliography*. Several years after that concrete demonstration that they had failed to make any where near a full literature search, they still claim they did. The point they make in the paper under consideration is that they think only competency tests can prove anyone can do anything scientifically reliable. If they really believed this, they would first undergo competency testing on writing law review articles critical of forensic disciplines before publishing such articles themselves. If they truly hold that their kind of reliability testing is required for admissibility of all expert witnesses, they would undergo such competency testing before testifying as anti-expert experts. If they were honest in their view, they would undergo competency testing to see if they can teach law on the university level before teaching, to see if they can do academic research before pretending to do it, and so on. If they do not voluntarily undergo the testing that they require of others, then it is from fear of failing, because fear of failure is the only motive they attribute to others who do not cater to their demands in this regard.

Let the reader not worry that any testing will ever convince these authors of anything contrary to their opinions. From page 43 to 63 they survey competency tests and views of their opponents on those tests. Read this section to learn how statistical twists can be placed on the same data in order to support either of contrary interpretations. If you need an excuse to reject any inconvenient research study, you will learn that by looking hard enough you can often find an excuse lurking within the study itself. Failing that, you will learn that you can dream up a plausible excuse and inject it into the study. Thus they consider a test in which FBI experts were involved. They suggest these were the FBI's best experts, then admit the results were impressive. They quickly create the excuse they need to reject the results out of hand: The FBI experts had motivation to do well while the control group did not. As law professors and authors of works on evidential law they should know that a purely speculative opinion is inadmissible because it is not probative, that no expert is qualified to read minds. The authors do both in stretching to reject what disproves their theory. This is a lesson for all who are compelled to reject undesirable fact.

Within 20 or so pages of review of competency tests, they come up with a few good ideas which could have come from the document examination literature they reviewed, particularly Albert S. Osborn. On page 52 they consider the idea that test results better than chance support the existence of expertise: "Is *chance* the criterion of expertise? If a driver manages to stay on the right side of the median strip more often than chance, if a piano student hits the correct notes more often than chance, if a student scores above chance on an exam—are they to be regarded as 'experts'?" [Emphasis in original.] Osborn went one better than that. He stated that an expert opinion is expert only if valid and acceptable reasons and reasoning are explained for it. Osborn (1935).

They discuss the advantage of a competency test being akin to real life problems. This, however, is balanced by their insistence on an a priori protocol to satisfy laboratory requirements of no extraneous influences. Thus every possible test will fail one or the other of these two sets of requirements, which they expertly explain makes every test they review unacceptable if nothing else does.

In their "Conclusions," at page 64 they point out the insistence of classical document examiners that a problem posed to the expert contain only the required information for the problem itself. I try to have attorneys and lay clients submit the case in that fashion. If one has a chance to explain the rationale, namely that later in the case the opposition will not have cause to insinuate a favorable opinion was responsive to the litigant's claims rather than the handwriting data, cooperation in that direction is almost always assured.

Their final segment, "Future Directions," has been shown to be far from perspicacious by subsequent developments. I want to address the assertion on page 65 that "at least one organization of document examiners has expressed interest in collaboration with empirical researchers in an effort to try to place their field for the first time, on a scientific foundation." Supporting research has always been there if the work was done correctly, in accord with the teachings and practices of the classical document examiners. It enjoys massive amounts of supportive research in fields with no interest in forensic handwriting expertise. I have authored surveys of research and other papers from medicine and psychology which directly validate assumptions and practices in forensic handwriting identification, Matley (2000) being one example. Many academic researchers today who presume to supply "for the first time" scientific bases for the expertise simply know and understand handwriting not much better than the authors of this critique. Worse, they exhibit no awareness of research reported in scholarly journals in several languages. The ignorance of some is never the measure of the knowledge of others, whatever conceit to the contrary which is entertained by the former.

They provide an appendix, "A Summary of the Principles of Handwriting Identification Theory." Principles, methodologies, and practices in demonstrative evidence are jumbled. To quote the King of Siam: "Tis a puzzlement." My only recommendation for their appendix is that, if you wish to become hopelessly confused on the topic, study it assiduously and take it seriously.

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begins segment: "The Twelve Causes of Individual Letter-formations." A thirteenth cause would be "psycho-pathological factors."]

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EVALUATION OF THE PUBLISHED DECISION:
United States vs. Starzecpyzel

880 F. Supp. 1027 (S.D. NY 1995) is the published opinion in the above cited case. Bear in mind that it is an opinion on the trial court level and would be superseded by opinions from Courts of Appeal and the Supreme Court which are at variance with it. That is my understanding as a non-attorney. Here we are concerned solely with the logic and good sense of the opinion in and of itself and with the rationale given for opinions of witnesses who are referenced in it. For simplicity the transcript of the *Daubert* hearing on which the opinion was based will be alluded to very little. Bear in mind that a judge can rule only on evidence presented by the parties. In this case we find that District Court Judge McKenna is subject to little criticism given the "evidence" experts for either party presented. The major flaw in his opinion is that he made universal statements based on a very fragmentary and flawed representation of forensic document examination from both parties' experts and of science from defense experts.

At page 1028 is a brief description of work by Gus Lesnevich, the government's expert at the trial itself. As I later wrote to Professor Risinger, they would have done better to challenge Mr. Lesnevich rather than the discipline of forensic handwriting identification. To illustrate the vulnerability of this often hired expert of federal prosecutors, see these items in the bibliography: BNA (1996) and Phillips (1995). In the case of *United States vs. Frame*, Eastern Dist. TX, Marshall Div., Docket No. 2:99-CR-2 (2000), that witness had positively identified defendant as writer of "the bulk of all" of some 2200 delivery tickets when hired by International Paper Co. to do so. At the first trial he was most assertive of the same opinion. At the second trial defense attorney pressed him: "Well, when you say it's a possibility, you're telling us in plain English, 'I can't tell you either way if he wrote or didn't write part of those tickets I examined.'" The admission was: "That's correct." The jury rightly acquitted on all 18 counts. If that witness had not changed his opinion there was waiting an expert who would have shown that his theory, method and demonstrative charts were in violation of established standards in forensic document examination and that many of his observations were in error. He got about $90,000.00 in corporate and federal tax moneys for that performance.

In *United States vs. Cusack*, Case No. 98 Cr. 691 (DLC) (S.D. NY 1999), Mr. Lesnevich had no hesitation in working for the federal prosecutor after having been retained by a private party to examine the same documents, after being denied permission in writing by that private party's attorney to hire out to the federal prosecutor, even hiring himself out prior to seeking such permission. In his testimony at trial he explicitly said that he had not done what he thought necessary to prove that any writing, which defendant was accused of forging and uttering, was in fact forged, that he had not proved

defendant to be the writer of any allegedly forged document, since he had only shown that defendant could have forged them. He had no hesitation in taking tens of thousands of tax dollars for testifying to such an opinion. In the parallel New York State civil case, *Cusack, et al., against 60 Minutes Division of CBS, et al.* (Supreme Court, County of New York, Index No. 600060/98), three experts and several lay persons submitted affidavits clearly impeaching every aspect of the man's "expert" testimony in the criminal trial. All this documentation and more will be available to the public on a web page set up by Mr. Cusack's civil attorney.

If action is brought against me for defamation or libel, I welcome the opportunity to have full discovery of the man and to present to a jury evidence regarding his claimed expertise. I have seen relevant documentation, including transcripts of testimony, and I have been personally involved in much of the cases referred to.

In the *Starzecpyzel* case none of the defense attorneys and defense experts challenged Mr. Lesnevich's testimony on the inherently implausible theories he espoused. For example, he asserted that individuality in handwriting results from a writer's changing the way he had been taught by the school model. The reader has perceived that, if this is so, the expert must demonstrate precisely what school model the writer in question was taught and exactly how the writer made changes in that style of writing which were habits at the time the questioned documents were made. Absent such a showing, an expert espousing such a theory cannot logically or technically say what the suspect did or did not write. You or I would have demanded that Mr. Lesnevich satisfy the demands of his own theory of handwriting identification, a thing which defense counsel in *Starzecpyzel* never did and a thing which the defense experts (collectively, self-proclaimed scientists in law, social psychology, research methodology, statistics, motor control and learning, psychology, exercise movement, and handwriting research) apparently never thought of between them. Yet they postured then, and may posture today, as competent to critique forensic handwriting identification evidence.

Mr. Lesnevich's highest credential is certification by American Board of Forensic Document Examiners (ABFDE), which considers itself the finest, if not the sole legitimate and credible, body certifying document examiners. A highly placed official of this Board, Mary Wenderoth Kelly, who at that time was instrumental in helping decide who was good enough for that Board's certification and recertification, was the government expert in the *Daubert* hearing. Thus, Judge McKenna was not presented with representation of what the entire field of document examiners in the country was doing, but only a scant 200 or so who were certified by ABFDE. Thus anything the decision says of document examiners should have been limited to ABFDE experts, if for no other reason than Ms. Kelly spoke as if only her people were doing it the right way as she

viewed it, the way to which she was testifying. At page 1028 the trial judge wrote: "The *Daubert* hearing established that forensic document examination, which clothes itself with the trappings of science, does not rest on carefully articulated postulates, does not employ rigorous methodology, and has not convincingly documented the accuracy of its determinations. The Court might well have concluded that forensic document examination constitutes the sort of junk science that *Daubert* addresses." Later the ruling is that, since document examination was not a science, it was a technical skill. Let us consider several logical errors of this judicial determination.

First, the Judge did not have evidence from the many hundreds of other document examiners in the country, but only from one highly placed official of one rather modest sized certifying body. Testimony was thus given only as to the theory and methods of its own 200 or less active members. Such particularized evidence does not justify a universal assertion such as the Court made. Second, if evidence showed that the school of document examination represented by ABFDE was, as later ruled, not a science but was junk science under *Daubert* guidelines, the Judge should have reasoned thus: "ABFDE methods and theory of handwriting identification expertise would constitute junk science expertise under *Daubert* if it were a science. But it is not science. Therefore, it is junk expertise. [Junk science – science = junk, not technical skill.] Any kind of junk expertise is inadmissible, under any judicial criteria. Thus, no ABFDE expert shall be permitted to testify in this case." That would have been both the correct logical, and the correct legal, conclusion given the evidence Ms. Kelly offered.

At page 1028 the two defense experts in the *Daubert* hearing are: "George Edward Stelmach, Professor of Exercise and Psychology at Arizona State University, and Michael J. Saks, Professor or Law and Psychology at the University of Iowa." One of the many failings of government counsel in the case was not to challenge these two to undergo a *Daubert* hearing at which they would have to satisfy all criteria they stated a handwriting expert had to satisfy. For example, they wanted handwriting experts to prove through published competency testing that such experts could identify handwriting reliably. If they honestly believed scientific experts had to undergo such competency testing, then they should have undergone it themselves before ethically offering themselves as experts. Of course, they never underwent such testing, nor has their vague expertise, which I call anti-expert expertise, received any recognition from anyone but themselves. It was a case of the worst occurrence of expertise which is established solely by the ipse dixit of the expert himself.

Professor Stelmach is typical of academic researchers who are literally in an ivory tower, but one which is well equipped with computerized toys. I give one instance of his "ivory tower thinking." He served up at the *Daubert* hearing his theory that studying

handwriting made on digitized, computer tablets would someday enable experts to identify real world writings, and the Judge uncritically gulped it all down. I have no doubt that most readers will say: "But have you ever established any link between what a subject does when writing in your laboratory under laboratory conditions on your computer digitized tablet and what the same subject does when writing real world documents with real pen and real paper?" It is obvious to anyone who stops to think for a minute that, unless such nexus is established before millions of grant money out of the public tax funds are spent on such laboratory studies, the money is wasted for everyone but the experimenter who gets to spend the cash, publish papers, give presentations at conferences and posture as an expert on handwriting at court hearings.

No one in the *Starzecpyzel* case confronted the Professor with this rudimentary indication that his research is scientifically pointless for the purpose he claimed for it. If Professor Stelmach had bothered to read the literature of the scientific study of handwriting for the past century, he would have discovered that researchers as Frank Freeman, Abraham Aaron Roback, Albert S. Osborn, Robert Saudek and June Downey researched handwriting, most of them by use of motion picture film of writers making real world writings. They then studied those films frame by frame. That is why they knew and understood handwriting, while Professor Stelmach, by his own admission, could only promise possibly to know and maybe understand something of it at some unspecified future date.

To show how researchers such as the Professor spend much time and funds in not quite finding out what every human knows, I quote from page 101 of a text he edited, Stelmach (1991): "As an example, it is possible to infer that the visual system that locates an object can be used not only to reach for the object but to also elicit verbal description where in space the object is located (Keele, 1990)." To translate this into English: One can guess that, if a person sees a thing, the person can both grab it and tell someone else where it is. These researchers are not sure of that; it is only possible for them to infer it. Do they not know that they do such things every day, particularly at the dinner table?

Daddy S: "Honey, where is the salt shaker?"

Momma S: "Right in reach of your left hand behind the mashed potatoes."

Daughter S: "That's okay, dad, it is six inches from my hand. I'll give it to you."

Daddy S: "That does not help me as I know of no published, replicated computer research saying you can see the salt shaker, tell me where it is, and know if you can take it and give it to me. Call the National Institute of Health for another $2,000,000 grant for me to study this problem in human movement and so take the first steps in creating a science of salt shaker analysis."

As for Professor Saks, the critiques I have given on the two law journal papers that he helped write more than expose his pretensions at being an expert on the merits of handwriting expertise. As a professor of psychology, he should have easily grasped the psychological aspects of the production of handwriting. If he ever did so, neither his testimony in *Starzecpyzel* nor anything in his writings which I have read give any hint of it. He should have researched the great number of papers on handwriting in the academic literature of psychology. Before he again presumes to testify as an expert about handwriting, it is suggested that he look in *Psychological Abstracts* and retrieve relevant papers. If he needs guidance, he may call me and retain my services.

At page 1035 the Court states: "Professor Stelmach explained to the Court that he considers himself a scientist because he publishes technical papers, supervises doctoral and postdoctoral students, competes for grants, and tries to 'participate in most types of scientific communities where there is review of hypotheses, where there is review of data, where there is scrutiny of methodology, and where conclusions are drawn.' Professor Stelmach stated that he employs the scientific method in his work, which includes 'peer review, scrutiny of methodology, scrutiny of error rates, and conclusions and interpretations.'" Note that most of this is ritualistic activity and nothing is said about advancing knowledge and wisdom or dissipating ignorance and stupidity. We have come a long way since scientists thought science (from Latin *scire*, to know) meant someone knew something for sure and understood it well enough to explain. Laboratory toys, communal rituals and jockeying for grant money now seem to be the hallmark of science.

When one reads research reports by "scientists" such as these, one is struck by an almost pathological avoidance of asserting that anything has been proven true or that research money was well spent because the researcher has learned something new. The promise of assured knowledge prompted the granting of money, but avoidance of asserting that such was attained allows the researcher to conclude that more research money should be given him for further studies of the same kind because the inconclusive results are so promising we might yet find out something worthwhile. The researcher must also disparage anyone rash enough to claim to know what the researcher admits not knowing, searching the claim for an excuse to reject another's knowledge, bidding for a new research grant to falsify what another has demonstrated. If, as in *United States vs. Paul*, the handwriting expert clearly demonstrates the reliability of the expertise, the anti-expert expert, who makes a living proving others know less than he, becomes unemployed. Similarly a researcher, who succeeds in discovering knowledge that the research is purportedly designed to discover, may fear becoming unemployed. Once the water comes in, who keeps on paying the well digger to dig?

At page 1035 is another example of judging others by one's own incapacity to understand what they said. Professor Stelmach is quoted as saying what he found "particularly distasteful from scientific terms" is that document examiners said they "did a global inspection," then "did a letter analysis." His evaluation is: "But they never state what they do." The following analogy may help him understand these two very specific descriptions of what they do. A commander of a regiment or division in action against the enemy will first review the entire front he commands and its relation to adjacent fronts. That is akin to a global inspection. Then the commander will ask for specific information on each unit in his command in order to be able to assess each unit in relation to the mission and present situation of his entire command. That is akin to a letter analysis, where the expert studies each letter of each word for the evidential value it has in the context of the entire document and in light of the problem to be solved. If the Professor does not understand these two basic functions in any analysis of a practical problem, it explains why he never realized that he must relate the laboratory writing which his subjects do to real world writing they do outside the lab; otherwise, if he should learn anything definite about handwriting from his research, it would be applicable only to other world, non-real, laboratory writing.

At page 1036 it is stated that Professor Saks' "testimony established that there is no strong statistical evidence supporting, or disproving, the 'two fundamental principles' or the reliability of forensic document examination." First, these "two fundamental principles" are neither fundamental to nor principles of identification. Second, statistical evidence of handwriting itself would be marginally useful, since handwriting is cultural and behavioral. The next moment when subjects of any statistical study write they may either deliberately or inadvertently alter the very thing which was tabulated. Third, those who require a statistical study to discover that the two government experts in *Starzecpyzel* are unreliable would be as inadequate to that task as they were to recognizing the fallacies which the two experts put forth concerning handwriting identification theory. Fourth, if these two experts were included in a study of the most competent experts, might they make the latter appear less competent? There is another consideration why the two Professors are wrong in saying that competency testing will prove whether handwriting identification is reliable.

When it comes to performance, only individual performers are competent or incompetent. If a test of 100 experts showed half were right and half wrong, it would not mean that handwriting identification theory and method are only half right. It means that half the subjects got it right and half got it wrong. An intelligent investigator would then drop the half who got it wrong and test the other half with a more difficult problem. Then those who got that wrong would be dropped and the remainder retested until one obtained

a group who always got the test problem right. One would then study what this group was doing and why, arriving at a proven methodology. The Professors are not into learning anything or establishing something; they are trying to prove everything false, for falsification is the heart of their scientific investigations. Indeed, in Professor Saks' world the best research leads to the worse outcome: "[T]he sloppier the research technique or no technique, or no real research at all leads to the most enthusiastic beliefs and highest confidence on the part of the people who do the technique.... And the more rigorously designed the research is ... the less enthusiastic the conclusions are." At page 1036. The exception is that the Professors' theories about science and research are themselves exempt from testing, rigorous or not, for they are merely communally held myths. This may be why, in Saks' words quoted above, he and Stelmach can be so enthusiastic for their ideas and so absorbed in their self-assurance.

Also at page 1036 the Court says: "Were the Court to apply *Daubert* to the proffered FDE testimony, it would have to be excluded." Due to massive confusion concerning what the Supreme Court really meant in *Daubert*, Judge McKenna should not be criticized for saying *Daubert* did not apply. He is to be commended that in this sentence he spoke of "the proffered FDE testimony," rather than all possible FDE testimony in the global ruling quoted above. However, since neither party asked the Court to consider whether FDE was a technical skill and since neither party offered any evidence supporting or refuting a claim that it was a technical skill, one could argue the Court had no jurisdiction to address that question, and, if it did, it had no evidential basis for any ruling on it. Since no admissibility had been shown, as was the government's burden as plaintiff, the ruling ought to have disallowed all such evidence. After the above quote, the decision becomes bogged down in an inconsequential discussion of that little bit of document examination literature that experts could bring to the Judge's attention, either because they knew little or because it was the little they could fit to their opinions. I believe that having a narrow view of what science and scientific research are would make one narrow in one's view of what writings are of scientific worth.

At page 1038 the Judge returns to a global appraisal versus appraising only "the proffered FDE testimony," saying the field only has the trappings of science, but that after *Daubert* it could not be considered a science. At page 1039 the Supreme Court in *Daubert* is said to have "appropriately focused on the 'scientific ... knowledge' branch of Fed.R.Evid. 702." The *Kumho* case clearly says no such separation was intended, that Rule 702 is one rule for all expert evidence, a thing no one knew for sure until *Kumho* came down. Indeed, *Starzecpyzel* says at page 1039: "While trial courts must always be concerned with the reliability of expert witness testimony, it is unclear whether *Daubert* provides, or was intended to provide, useful guidance for nonscientific expert testimony."

At page 1040, the root of all scientific silliness propounded by defense experts is identified, but unfortunately taken seriously: "The essence of *Daubert's* 'reliability' standard lies within the Court's citation to philosopher of science Karl Popper's statement that 'the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability.' — U.S. at —, 113 S.Ct. at 2797 (quoting Karl Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge* 37 (5th ed. 1989))." Popper's theories themselves are neither falsifiable nor refutable nor testable, in the way he and those who believe in his myths think scientific theory ought to be. Therefore, by Popper's own measure, his theories about science are not scientific. They are, therefore, self-falsified, self-refuted, self-tested and found wanting, because they are inherently self-contradictory. Only an irrational person, or one who had not thought the matter through, adheres to the self-contradictory. The virtue of the Popper theory is to make science truly democratic, because now everyone can play the college credits game, follow rituals, avoid too much logic and learning, write a slick grant proposal, work connections into the scientific ruling class, falsify something, and so become a scientist.

Continuing efforts at finding a way to let the government's expert testimony in, the Court pretty much runs aground in comparing harbor pilots to the FDE. Professor Starrs suggested that harbor pilots could well take offense at the comparison, Starrs (1995). Even *Webster's Third International Dictionary* is enlisted in the effort to rule for admissibility. The Courts's reasoning, covering five or six pages, has been swept away by *Kumho*, a thing we should be grateful for. The pieces of the reasoning of interest to us are those which still haunt courts of law, as they have haunted courts of law probably since the first expert witness appeared in the first court of law.

At pages 1043-1044 it is stated: "Presented with a forgery detection problem, FDEs conduct a two stage analysis. First, FDEs scrutinize the genuine and challenged exemplars and identify 'significant' similarities and differences. Second, based on their training and experience, FDEs combine their first stage observations and draw inferences as to the genuineness of the questioned signatures." Once we have identified and exposed each separate error in this statement, the whole will implode.

1. The two stage analysis given is only recommended if the "expert" wants to be sure the conclusion fits the client's contention. See my reply to Professor Moenssens at paragraph 40 for a brief description of proper steps in a proper analytical method. The disputed writing and the genuine writings which are to be compared must be scrutinized, to use the Court's term, separately. Their uniquely identifying characteristics must be established in isolation from each other. Then the characteristics are compared. The writings are compared directly only as an adjunct or completion or testing of the comparison of characteristics.

2. As my discussion above shows, there are some dozen steps in a proper analysis, steps which might have to be repeated and reviewed before an opinion is finalized.

3. The word "significant" was put into quote marks by the Court, presumably because no one could explain to the Judge what constituted significant characteristics for an identification of handwriting. In summary, it is so simple that only the simpleminded cannot comprehend it. A significant trait is one which is either too inconspicuous to be noticed and thus not subject to deliberate disguise or imitation, or one which is so difficult to alter or imitate that one cannot do so or, if one manages to do so, the effort at continuing breaks down fairly quickly. These are established by experimental methods, described by Saudek as he employed them. Saudek (1929, 1978). The drawbacks of Saudek's methods are that they require neither a $2,000,000 grant nor sophisticated computer equipment and that they result in definite knowledge validating forensic handwriting identification.

4. FDEs are said to base opinions on their training. That does not support the Court's decision to admit the evidence, since there was no showing that any specific training resulted in any specific skill to form opinions. Indeed, the *Daubert* hearing did not explore anything specific in the training, only generalized characterizations such as a two-year apprenticeship, reading books, and so on. Certainly defense did not have enough good trial sense to explore specifics of Mr. Lesnevich's training and follow it up with an investigation into his claims.

5. FDEs are said also to base opinions on experience. That does not help support any ruling on admissibility without a showing that experience itself was reliable work resulting in habits of reliable work. Unless the quality of experience is shown, mere experience is like a magician's box into which a lovely assistant is placed only for the box to be empty when later examined. Expert experience is a magical box into which all things are said to have been put but from which nothing concrete and specific is ever drawn. See paragraph 6 of my reply to Professor Moenssens for a fuller discussion of experience as a spurious qualification proving expertise.

6. FDEs "combine their first stage observations." However, those observations are made by a method virtually guaranteeing a biased and incomplete set of data, no matter how earnestly the expert might wish to be honest and objective.

7. FDEs are said to draw inferences from a combination of experience, training and combined observations. One logically draws a valid inference only from a major premise stating a precise theory of interpretation and a minor premise consisting of verifiable and demonstrable observations. For an excellent explication of correct expert reasoning, see Imwinkelried (1988). The amorphous major premise of training and

experience makes any inference by the expert illogical and its acceptance by others irrational.

The Court asserts: "Defendants have made no showing that either stage of this analysis is likely to be faulty, e.g., that no detection of significant differences between writings can be performed. Defendants have simply challenged FDEs to meet a scientific level of proof that such skill exists." That is not quite correct. The proponent of evidence carries the burden of proving its admissibility, while the government and its *Daubert* expert did not meet that burden. The defense did positively prove that the specific proffered testimony of an ABFDE expert did not meet the defense theory of what scientific testimony ought to be. The Court accepted that theory. Neither party offered any legal theory that the proffered FDE evidence was other than scientific. That Mr. Lesnevich had once testified before Judge McKenna that it was an art based on science was not evidence before this Judge in this case in this *Daubert* hearing. The Judge presented both the legal theory and the key factual evidence upon which he based his ruling to admit otherwise inadmissible evidence, inadmissible given the theory of admissibility he had adopted wholesale from defense experts.

Starzecpyzel is viewed as a great defeat for document examination. Contrariwise, it was winning a war after having lost all the battles. It was like the time when the United States Men's Olympic Basketball Team lost the gold medal to Team Soviet. Officials kept extending the time of the game, permitting Soviet players to throw the ball in from the sidelines and have their best player try for a basket until, at the third or fourth try, he made it. The game was promptly terminated. Judge McKenna let the defense win the *Daubert* game until the very last second, at which time he himself scored the winning basket for the government. The Judge scored several last minute baskets to assure a solid win for the government.

Besides offering his own testimony as to what Mr. Lesnevich had said in a previous case, the report states at page 1044: "Although Ms. Kelly was unable to explain to the Court's satisfaction precisely how 'significant' similarities or differences were identified, the Court has no doubt that such identifications can be performed, in some cases by cursory examination." For evidence supporting the Judge's own expert testimony on the point, an illustration from the 1958 edition of Harrison's book is reproduced at page 1045. The Judge goes on scoring point after point in favor of admissibility, none of it based on any evidence received in the *Daubert* hearing.

At page 1046 are examples of some astute reasoning by the Court. This Judge was sharper than anyone else involved in the *Daubert* hearing. What could he have done with proper presentation of this expertise! In any case, the following are examples of several excellent points:

1. "[T]he Court concludes that conclusions as to genuineness can be reliably drawn, where, for example, one FDE documents a large number of significant differences, and an opposing FDE can neither rebut those differences, nor present a large number of countervailing 'significant similarities."

2. In discussing comparison between handwriting and fingerprint analysis: "Fingerprint analysis, however, is a far simpler task than handwriting analysis. Fingerprint patterns contain no 'natural variations,' are unaffected by such factors as disease, intoxication and the passage of time, and do not easily permit purposeful disguise."

3. Speaking of highly individualistic writing: "Where such individuality is present, a *single* significant difference may suffice to indicate a forgery." [Emphasis in original.]

4. Having accepted the invalid, two-stage analytical method Ms. Kelly described and Mr. Lesnevich used, the Judge wisely notes at page 1047: "While jurors can visually confirm an FDE's first stage efforts [i.e., comparison of the two writings], no ready verification of the second stage results, the opinion as to genuineness [based on experience and training], is possible." However, we can counter that with proper method the expert would explain the interpretative theory applied to the observations; and, if it does not make good sense, an intelligent juror would reject it. That underscores the advantage of the method presented in *Starzecpyzel*: The fact finder is forestalled from finding the fact of a magician's trick box.

On page 1047 is an assertion that, absent any ready verification of the bases of the expert's inferences, "reasonable reliance on the expert, rather than formal proof by the expert, will often inform the fact-finder." The expert now becomes the major premise of his opinion, so that his ipse dixit is sufficient reason to accept his opinion and so convict the defendant. The Judge effectively ruled that it is reasonable for the fact-finder not to find fact but accept the word of a hired witness for the prosecution. The expert is in effect the ultimate jury, a situation inviting expertise for hire. I am convinced that the only way to have a jury make an objective evaluation of any kind of expert testimony is to have all qualifications heard by the judge outside the hearing of the jury and forbid any mention of qualifications in the presence of the jury. The jury should hear only the observations, the demonstrations for the truth of the observations, the scientific premises for interpreting the observations, and the logic leading to the conclusion. One speaker, an expert on being an expert witness, gave a presentation centered on the concept that it was a dog and pony show. Personal posturing with both dog and pony must be banished from the courtroom if we want justice and not trial by entertainment and expert one-upmanship.

Having decided an FDE's testimony is unscientific but yet admissible, the Court created an instruction which would protect the jury from accepting as scientific something, the bases of which they could never grasp but would accept on blind faith in the expert's assertions of his own righteousness as a witness. Last of all is addressed the nine step statement of probability which is standard in expert handwriting identification. At page 1048 it is stated: "No showing has been made, however, that FDEs can combine their first stage observations into such accurate conclusions as would justify a nine level scale." The irony of the rejection by the Court of this scale is that it exactly parallels the levels of proof required in courts. If expert evidence cannot fit into this scale, nor can it fit into the scale setting various levels of burden of proof which litigants must meet. Since a criminal conviction must be based on proof beyond a reasonable doubt, the sum and substance of the *Daubert* showing by both government and defense experts was that the ABFDE expert could never prove anything beyond a reasonable doubt. Having to accept an expert's opinion ultimately on that expert's claim as to what his training and experience were and his claim that they support his and no other conclusion, is both unreasonable and doubtful as a level of proof.

The decision ends with tossing a bone to the defense. They could bring all their *Daubert* "evidence" into the trial itself and bring in their own FDE. One could ask how they might do the latter. Having demeaned the breed, one would not marry one and bring it into one's own home. What they needed was an honest, competent, knowledgeable FDE who as a trial consultant could teach them how to impeach the inadequacy of the government trial expert's theory and methods. Their own *Daubert* experts exhibited no capacity to do what was needed in that regard. One wonders about the outcome of the trial, something not discussed in this much discussed case. It is as if the central drama of defendants' guilt or innocence became an inconsequential aside. That in my view was the most inexcusable fault of the defense experts. If they could not as experts have proven defendants innocent of the making or uttering of the allegedly forged writings, they should have served as consultants on how to force the government to make a legally and constitutionally sufficient proof of the charges. They could do neither of those things and thus had no business using defendants as experimental subjects in promoting a newly fabricated legal theory of admissibility, a theory proven fallacious by later court rulings in the *Kumho* and *U.S. vs. Paul* cases cited earlier.

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Stelmach (1991). *Motor Control of Handwriting.* 5th annual conference of the International Graphonomics Society, October 27-30, 1991, Tempe, AZ. Edited by George E. Stelmach.

The theory upon which these expert witnesses are permitted to testify is that handwriting is always in some degree the reflex of the nervous organization of the writer, which, independently of his will, and unconsciously, causes him to stamp his individuality in his writing. I am convinced that this theory is sound, but at the same time I realize that in many eases it is unreliable when put to practical test. It must contend not only with disguise, but also with the influence of possible abnormal mental and physical conditions existing when the writing was made—such, for instance, as the position of the body, whether reclining, sitting, or standing; the height and stability of that upon which the writing rests, and the character of its surface; the character of the paper written upon; the ink; the pen, and bolder of the pen; the health of the writer's body and member with which the writing is made, not only generally, but also with reference to the accidents and influences or the moment. It follows that unreliability is greater when the disputed writing is short, or the standards for comparison are meager, or are all written at one time, and also that uncertainty lessens when the disputed writing is long, and the standards are numerous and the products of different date. Handwriting is an art concerning which correctness of opinion is susceptible of demonstration, and I am fully convinced that the value of the opinion of every handwriting expert as evidence must depend upon the clearness with which the expert demonstrates its correctness. That demonstration will naturally consist in the indication of similar characteristics or lack of similar characteristics between the disputed writing and the standards, and the value of the expert's conclusion will largely depend upon the number of those characteristics which appear or are wanting. The appearance or lack of one characteristic may be accounted to coincidence or accident, hut, as the number increases the probability of coincidence or accident will disappear, until conviction will become irresistible. Thus comparison is rated after the fashion of circumstantial evidence, depending for strength upon the number and prominence of the links in the chain. Without such demonstration the opinion or an expert in handwriting is a low order of testimony, for, as the correctness of his opinion is susceptible of ocular demonstration, and it is a matter of common observation that an expert's conclusion is apt to be influenced by his employer's interest, the absence of demonstration must be attributed either to deficiency in the expert or lack of merit In his conclusion. It follows that the expert who can most clearly point out will be most highly regarded and most successful.

In re Gordon's Will, 50 N. J. E. 397, 26 At 268, at 277 (Prerogative Court of New Jersey, 1893)

COMMENTS ON ANDRE MOENSSENS' PAPER: "HANDWRITING IDENTIFICATION IN THE POST-DAUBERT WORLD"

The following text was, as the addressee portion shows, sent as a letter to the editor with a copy to Professor Moenssens and copies to editors of several legal journals and newspapers. I am not aware of any having published it, while neither the Professor nor the journal in question acknowledged receipt of the letter. The Professor told me at the 2000 conference of American Academy of Forensic Sciences that he had received it but it was too long to read or reply to. The letter is unedited from its original form.

September 21, 1998

AN OPEN LETTER TO:
The Editor, UMKC Law Review
University of Missouri at Kansas City, Law School
5100 Rockhill Road
Kansas City, MO 64110

COPIES TO: Editors of other legal and forensic periodical publications.

Dear Editor:

In your Volume 66, Number 2, Winter 1997 issue, there was a paper by Andre A. Moenssens titled *Handwriting Identification Evidence in the Post-Daubert World.* I wish to offer a few general comments followed by some specific comments. The general comments are:

1. Professor Moenssens, whom I shall henceforth refer to as "The Author," makes a commendable effort at defending the profession of questioned document examination (QDE). Unfortunately, he opens wide gaps in the frontiers, inviting further attack from outside. Some examples will be discussed later.

2. The Author repeats several of his points several times. Only once is required, and that once is best made in the most logical place for it. Without these repetitions, the paper would have been about 20% shorter.

3. The Author makes astute comments on the mistakes of those who attack handwriting identification, whom I will refer to as "the critics." Then he makes some of

the same mistakes himself, of which some instances will be mentioned. As to the major journal paper by the critics, I shall refer to it as *Exorcism*.[1]

4. The Author puts the bulk of his comments in footnotes. Either the comments in footnotes are worthwhile parts of the paper's theme or they are not. If they are not, why have them? If they are, why not integrate them into the text? It is understandable to have a few footnotes for explaining collateral but important points or for referring readers to related discussions and to source materials. To have the body of his paper be the decidedly smaller portion seems topsy-turvy.

5. Lastly, he has lengthy discussions ranging from background through incidental to the irrelevant. Such excursions could have been omitted with no loss to the reader.

Now, some comments on specific portions of his paper.

6. At page 253 and several other places, The Author talks of "extensive experience" and how it qualifies the expert. There are two difficulties to reliance on experience to qualify an expert. First, for document examiners who ipso facto merit his disapproval, no amount of experience apparently redeems them. Second, it is unreasonable to take experience as a qualification unless the quality of the experience is demonstrated. Experience at being a bungler will not make one anything more than an expert bungler. Experience making a mistake repeatedly will not make one mistake proof. If experience is a criterion for accepting an expert's opinion, then all an opponent need do is obtain an expert with one more day's experience. Again, if experience makes one an expert, how much experience? Five years? If so, how about four years and 364 days, or whatever the precise number of days needed? The very question demonstrates an absurdity in the premise that "enough" or "extensive" experience qualifies an expert.

7. In footnote 4 at pages 253-254, The Author remarks on the difficulty of obtaining factual information about many handwriting groups, apparently those groups of which he disapproves as actual or potential document examiners. Did anyone think to contact the various groups? Perhaps prejudice cannot survive exposure to accurate information.

8. At page 255, The Author begins his consideration of the critics' contention that handwriting identification is not scientific. He does not offer his own definition, though he is right in rejecting the critics' idea of what science is. Among other things, their ill-defined notions would permit perfect stupidity to be ranked as science as long as it had all the trappings they value so much. To define "science" by reference to scientists, as some do ("science is what scientists do"), is invalid, since "scientist" must be defined in terms of science. I submit that science is not a thing in itself, but a quality of a person's mind. If this same quality is shared by a number of associated persons, we recognize a particular scientific community. A science is a special knowledge, "certain and evident knowledge

acquired by reasoning strictly from indisputable principles," an "intellectual knowledge that is certain, and evident," the opposite of belief and estimate on the one hand and ignorance and simply not knowing on the other.[2] It is knowledge of things in terms of their causes, giving understanding of their occurrence and functions, as well as wisdom in making judgments about them.

A particular science studies a particular class of things under a special aspect, and it is characterized by both of those elements. Thus, handwriting identification studies handwriting, a subject it shares with other disciplines, but it does so under the special aspect of what identifies the writer. It has established the factors which account for the specific style of writing any person achieves, and it ascertains specific causes and influences during the actual making of handwriting. Besides these principles which handwriting experts have established through experimentation, the discipline also takes axioms and tools established by other scientific communities, such as microscopes from optics and rules for measuring from mathematics and geometry. Finally, handwriting identification performs its work in accord with scientific analytical procedures. Therefore, handwriting identification is a science within the individual who has achieved proper knowledge and understanding, and it is a scientific endeavor performed by the individual who applies this knowledge correctly to appropriate practical problems.[3] With this definition of science there is no room for those attacking handwriting identification to claim that their attacks are scientific. In this regard, The Author scores his most telling points.

9. At page 256 in footnote 14, the author says that "most historical accounts are irrelevant to the current legal controversy of whether handwriting identification" is scientific knowledge or not. Au contraire. Its science was firmly established by historical figures who did primary research over the past hundred or so years and by their arduously won victories in having courts of law recognize their scientific status.[4] We are in trouble precisely because we failed to master our inheritance of handwriting knowledge and wisdom and to enrich it.

10. Beginning at page 256 with the section titled "There are Document Examiners and ‘Document Examiners,’" The Author makes an error he convicts the critics of. This error is to separate his preferred kind of document examiner from all others but to lump the latter together. His criteria for the separation are three-fold: belong to an organization which has his approval, and, less universally applied, work for some government agency, but, by rigidly stated policy, not be a graphologist. He condemns, sight unseen and word unheard, any expert lacking these three wedding garments to be cast out of the forensic banquet hall into the darkness beyond. With the mark of the

graphological Cain upon them, such souls are forever damned to exclusion from the ranks of the very few Chosen Few.

He justifies his blanket condemnation by stating that all graphologists shall be considered to be a single graphologist whom he describes as the epitome of incompetence. This is precisely the central fault he rightly convicts the critics of, since like them he paints with a broad brush to untempered purpose all those whom he wishes to discredit. On the contrary, some to whom he attributes confirmation in perfection have erred, even in major international cases.[5] This fact does not tarnish their associates, but only shows that all document examiners are fraught with human frailty. Only the generous acceptance of the best contributions to be had from all of us will achieve the best this profession is capable of. Whitewashing the mistakes of the politically acceptable merely waters the seeds of future judicial rejection.

11. At page 257 in footnote 15, The Author mentions *Journal of Forensic Identification* as a dominant journal for publishing questioned document papers. A friend shared his file with me. There were only 11 items specifically on questioned documents in more than 48 issues. The journals which The Author brushes aside are worthier of acknowledgment. Here is a small appetizer plate of their quality offerings:

(a) John S. Gorajczyk. *Reverse Thin Layer Chromatography for Ink Examinations.* 1 JOURNAL OF FORENSIC DOCUMENT EXAMINATION, 5-12 (Feb. 1987).

(b) Richard W. Chang; and Michael M. Zanoni. *The Value of Cancellation Marks in Handwriting Identification. A Case Report.* 1 IBID., 31-3 (Feb. 1987).

(c) Thomas Black. *Response: University of Pennsylvania Law Review Article, "Exorcism of Ignorance as a Proxy for Rational Knowledge: The Lessons of Handwriting Identification Expertise."* 3 IBID, 38-40 (Fall 1990). [Contrary to The Author's contention, those branded collectively as unqualified were not gleeful about *Exorcism* and did mount early responses.]

(d) And a goodly number of original handwriting research projects reported in the same journal and in the following three journals.

(e) Carl E. Anderson. *The Chemistry of Handwriting.* 2 JOURNAL OF QUESTIONED DOCUMENT EXAMINATION, 18-30 (March 1992). [A medical doctor discusses the neuromuscular dynamics of handwriting.]

(f) Robert L. Kuranz; Sue Iremonger; Joe Nickell; and Kenneth W. Rendell. *The Continuing Saga of "Jack the Ripper Diary."* 6 IBID., 3-9 (Fall 1997).

(g) Jacqueline Joseph. *Identifying the Maker of Numerals.* 19 NATIONAL ASSOCIATION OF DOCUMENT EXAMINERS JOURNAL, 7-11 (Spring 1996).

(h) Emily Will. *Medical Records Case Solved Photographically*. 21 IBID., 9-14 (Spring 1998).

(i) Marcel B. Matley. *The Vincent Foster "Suicide Note." Did Strategic Enterprises' Purported Experts Prove it Forged?* 21 IBID., 15-30 (Spring 1998). [A defense of government examiners who had it right, while experts associated with American Board of Forensic Examiners had it very wrong.]

(j) Mary I. Duncan. *Characteristics of the Writing of the Blind and Visually Impaired*. 152 WADE JOURNAL, 3-15 (June 1992).

(k) Barbara Shipper. *Identification of Computer Printouts*. 220 IBID., 3-8 (March 1998).

12. At page 258, The Author says: "While the term 'handwriting identification' is almost a household term, the task of comparing handwritings constitutes but a small part of the varied workload of questioned document examiners." Not so. It comprises 80-90% of the tasks performed. That was reported in 1958[6] and in 1995,[7] which makes it a remarkably stable statistic. (He does somewhat retract the misconception on the next page.) For that reason I choose to emphasize the scientific knowledge which our and several other disciplines provide about handwriting. There is no course or school to teach this comprehensive science save academic libraries and self-application.

13. At page 259, we read: "Experts engaged in these sophisticated analyses may require extensive laboratory equipment." This is partially pretentious. For example, paper watermarks require neither sophistication nor laboratory equipment. Hold the paper up to a bright light source, check directories such as Lockwood-Post and write to the manufacturer. Watermarks are put into paper so that even the unsophisticated can verify authenticity of the paper. Much of the laboratory equipment used in QDE requires minimal intelligence and skill. Things like ESDA (electrostatic detection apparatus) can be figured out by any high school graduate reading a two-page instruction sheet on one's own, most of the two pages taken up with illustrations. Those specializing in such things as the chemistry and physics of paper, toner and ink do indeed use highly technical equipment and most have advanced degrees.

14. At page 259 and elsewhere, The Author offers his explanation of what graphology is all about. The result is as if one were to describe "citrus fruit" only in terms of the lemon.

15. In footnote 27 at page 260, the dissenting opinion in *Hooten v. State*, 492 S2 948 (MS 1986), is cited. The majority said it was error for the trial court to exclude a graphoanalyst as a handwriting expert. When one considers that the lone dissenter misconstrues some things to make his point, one becomes wary of his entire argument. For example, at page 958 he cites *State v Anderson*,[8] to support his argument that a

graphoanalyst does not qualify as a document examiner. As the dissenter himself noted, that was not was a questioned documents case; nor did it even discuss the proffered expert's qualifications. Thus it was doubly irrelevant to the issue it was cited to support. In *Culbreath v Johnson*, 427 S2 705 (MS 1983), the same Supreme Court as in *Hooten* stated that the trial court was correct in agreeing with the *Hooten* graphoanalyst and that it itself also agreed. Surely it was an oversight by The Author and his researchers not to include the *Culbreath* case as a fairness in balanced scholarship.

If a graphologist must be barred from becoming a document examiner, must a penmanship teacher (such as Albert S. Osborn), or a doctor (such as Julius Grant), be also barred on the same logic? Or ought anyone having a prior occupation be ipso facto barred from entering any change in one's profession? Might not American Academy of Forensic Sciences (AAFS)[9] and its kindred questioned document groups have many members with prior occupations which have far less relevancy to handwriting expertise than handwriting analysis?

I wrote a paper surveying reported court cases which considered graphologists and graphoanalysts as expert witnesses, evaluating the reasons why some failed while many so often succeed.[10] One could write a paper of greater length delineating embarrassing rulings regarding experts from the "school of thought" of which The Author approves. Why do that, unless it be for the positive purpose of learning from the incidental and unfortunate mistakes of our fellows?[11]

16. In footnote 30 at page 261, three graphologists are mentioned by name. Shirl Solomon is a lady whom The Author refers to as a man. Roger Rubin follows the finest European traditions in handwriting analysis. I am gratified that The Author graciously indicates how one can obtain Sheila Lowe's software. She is an altruistic and dedicated professional. But notice the gratuitous, unfounded association made between these three people and the case discussed in paragraph 15 above.

17. At page 263, it is stated that "serious improprieties began to occur" when graphoanalysts entered QDE work, which would not have occurred had they "received adequate training." First, is it contended that no serious improprieties ever occurred until such entry or that only graphoanalysts are ever guilty of them? Second, what is considered by The Author's select groups as adequate training is their own and no one else's, a presumption which can be appreciated if one imagines that the Ivy League Colleges were to claim that only their schools offer adequate higher education. Third, any of us could use case law, the professional literature and personal experience to document occasions when some of The Author's "adequately trained" experts made serious errors and improprieties. Would such events say anything about the organizational associates of these experts? Not at all! Nor does allusion to alleged,

unspecified impropriety by some unnamed graphoanalyst say anything at all about any other person who might be or have once been a graphoanalyst.

18. At page 264 in footnote 39, The Author refers to those refused admission to "legitimate" organizations, as if organizations he disapproves of were illegitimate. I once applied to International Association for Identification for membership with sponsorship of the late Raymond T. Moore. I was refused for one reason: Knowing graphology, I lacked the requisite ignorance. What The Author has swallowed whole, and wants judges and attorneys to swallow uncritically also, is this: Ignorance of one discipline of handwriting analysis is required in order to know another. The more one knows of any related discipline (paleography, medical and psychological research in handwriting, teaching of penmanship, the fine art of formal writing, OCR, and several others), the more one is adept in one's own field of forensic handwriting examination. On the other hand, the more one cultivates ignorance, bias and ill-will towards fellow humans in a related discipline, the less qualified one is in one's own field.

19. At page 266, The Author says "we are not yet in the future," as to those he disapproves of seeking "more traditional approaches to handwriting identification." The future has passed him by! Many of us in the field reach back into the 1800's and embrace all available literature up to this day. It is by mastering both traditional and advanced approaches, and by recognizing and respecting one's professional limitations, that document examiners can act competently and ethically.

20. The case *U.S. v Starzecpyzel*, 93 Cr 553 (LMM), 880 FS 1027 (S Dist NY 1995), is mentioned several times. The author ignores the fact that the key problem was that the chairperson for certification by American Board of Forensic Document Examiners (ABFDE) was the government's *Daubert* expert. That person could not describe her duties as certification chair, nor say how many people were certified or what was needed to be recertified.[12] Several times the question was put about inter-writer variation, and the answer was an explanation of intra-writer variation, even after the judge carefully explained the difference. There was no apparent awareness of the massive amount of primary research into handwriting identification which has gone on around the world. The judge had no choice but to rule on the basis of the evidence before him that handwriting identification would be junk science if it were a science. Unarguably, the description of our discipline which the certification chairperson of ABFDE gave was of a most unscientific endeavor. Yet that person's performance really says nothing about the other 100 or so persons whom ABFDE has certified over some 20+ years. The original grandfathered group were not genuinely certified; to be grandfathered is to be excused from having to be certified.

21. At page 270, The Author states that only 300-500 individuals could satisfy the requirements of his approved organizations. Is it seriously contended that only about 1 out of every million Americans can qualify as a handwriting expert? If we discount those who were grandfathered rather than qualified, it is perhaps only one out of several million in all of North America. No document examiner has met anywhere near the educational, testing or certification requirements of Board Certified physicians. Yet they far out-number a mere 300-500 document examiners. Lawyers undergo more rigorous training and more rigid testing than any of us have. California alone has about 129,880 active attorneys, more than 200 times the alleged number of qualified document examiners for the entire country. What is one to infer if one is reasonable and perceptive? That the "rigid requirements" are actually a closed shop.

22. The Author engages in belittling insult which does not come out with a good old fashioned, direct insult. One example of several possible will do. At page 270 in footnote 71, he says of an unapproved publication that it has a "professional-sounding" name. The insinuation is that the journal is not professional, that it only pretends to be. Since I subscribe to it, I can assure the reader that content, typography and style of writing are professional.

23. While his acceptable experts are carefully distinguished from anyone having any graphological experience, there is one fact which will be disconcerting to those who must maintain prejudice in the face of reality. A couple of years ago I checked the membership of the QDE sections of IAI and AAFS against what graphological rosters I had available. Between them approximately 10% of members had such connections.

24. At page 273, The Author falsely states that "graphological groups" failed to respond to the *Exorcism* article. (But see item (c) in paragraph 11 above.) I wrote a letter to the editor of the *University of Pennsylvania Law Review*. They sent rejections that year and the next, maybe to make sure. The letter was issued as a monograph.[13] Document examiners of a graphological background were delighted that all examiners, of whatever persuasion, were being defended. It went beyond that. Professor Risinger ordered three copies of the monograph, which were sent along with a gift, a monograph annotating original research papers published in standard forensic journals and written by document examiners to demonstrate the connection between health factors and handwriting.[14] Whereas *Exorcism* had said nowhere in the Boston area could QDE journal material be located and that there was no research in handwriting identification, masses of material were uncovered in the Greater Bay Area. After the *Starzecpyzel* case, Professor Risinger had Seton Hall Law Library purchase *QDE Index*,[15] as the government attorney had cited it to back up the scholarly status of our discipline.

25. I am pleased with the assertion about what the ulterior motives of the critics are. While we take that as a weapon to defend ourselves, it must also be a caution lest any of us slip into the same ulterior motive. If highly endowed individuals can make such mistakes, which of us are immune?

26. In footnote 124 at page 283, we read a quote: "Under the scientific method [as articulated by Newton], a scientist proposes a hypothesis and then engages in experimentation or observation to validate his hypothesis." If a critic testifies in court that this is what handwriting experts must do, simply ask on cross-examination: "Where, Mr. Expert Critic, does the hypothesis come from?" It has to come from a previous investigation of reality, which is the physical foundation of a physical science, while intelligent curiosity guiding the investigation is the mental foundation. The hypothesis is only one step, not always a necessary step, in the scientific method. For example, could you imagine a research project in hydraulics to determine whether or not the "hypothesis" that water flows downhill is true or not? Who knows whether or not, when the critics forget to shut off the bathtub spigot, they return to find water all puddled on the ceiling and draining out their ceiling vent to the sky above?

27. At page 287 and elsewhere, the idea that peer reviewed periodical literature is required for a thing to become scientific is mentioned. If true, this conclusively proves that the critics are unscientific. Never has it been published in peer reviewed journals that publishing in peer reviewed journals is required for science, after the hypothesis was researched, statistically analyzed, and peer reviewed before publication, then subjected to published replication. Therefore, the very premises of such a theory are unscientific by its own terms.

Also at page 287, Popper's idea of falsifiability is mentioned, an idea that critics love to throw in the face of handwriting expertise.[16] Making falsifiability a keynote of science assumes that a thing cannot be verified. Did Popper ever test the falsifiability of any of his peculiar notions, including that of falsifiability being a keynote of science?

28. At pages 291-292 are suggested criteria for testing reliability. None get down to brass tacks in the particular problem at issue. Criterion 4, a program of education and training, conclusively proves that The Author's preferred document examiners are unreliable. After 75 years or so they have produced but 300-500 experts who alone are supposedly properly trained and educated. Actually, far fewer than that, since many of the 300-500 are among the original founders of training organizations.

29. For some reason, there is an extended discussion why ear identification is nonsense. In several respects it reminds one of the attack against QDE. Even if the ear specialist cannot identify whose ear made a particular impression, he can perform what at

times might be an even more vital service: The elimination of many an innocent ear from suspicion.

30. At page 301 in footnote 223, it is stated how ASQDE circulated its alleged research within a closed circle of 100 people. If a supposedly scientific organization does not open its work to public scrutiny, it is not scientific. I wrote to the president of ASQDE about subscribing to their journal. He never replied. It is of no moment if one produces supportive literature for public performance, but keeps that literature private or semi-private. If it is kept private, it is preposterous to claim public credit for it. *A scientific community must be an open community*, while a closed community engenders closed-mindedness.

31. In footnote 231 at page 303, it is mentioned that *Exorcism* authors failed to note that numbers of graphologists had "invaded" the ranks of document examiners. This speaks of us as if we were the Golden Horde in the Dark Ages rushing out of the Eastern steppes or the menacing "Yellow Peril" moving into California in the late 1800's. *Exorcism* based its entire "factual" argument on the bungling of *some* government examiners. This is the minority type of handwriting expert whose reports and testimony do not disclose patterns and complexes of traits. It is mostly pick isolated things here and there, using the ancient, discredited comparison by formation.[17]

32. In various places The Author does a masterful job of illustrating mistakes in citation and research by the critics, and of the singular kind of testimony they give. The paper would have been better organized had all such material been placed in the body of the text in proper sequence, with all of each kind of demonstration gathered in one place. It is almost as if many cogent arguments were stuck in footnotes here and there because they were thought of randomly after the paper was finished. I do not say that is what happened, only that the loose organization gives that impression.

33. In footnote 248 at page 307, Robert Saudek is mentioned. To put it bluntly: If one does not know Saudek's *Experiments with Handwriting*,[18] one does not know handwriting. Period. All criticism which I have seen of Saudek illustrated the misunderstanding the critic had of the book. In the Secret Service course, the section on anonymous writings provided the most perceptive treatment of handwriting. It is all taken from Saudek's *Experiments with Handwriting*, but without proper attribution.[19]

34. In footnote 204 at page 297, we read a quote from the *Daubert* court: "Arguably, there are no certainties in science." The best reply to that assertion is to ask: "Are you certain of that?" If the response is "yes," then there is one certainty in science. If the response is "no," then the contrary is certain. In either case, there is necessarily some certainty in science. If we take a legal rather than logical viewpoint, a forensic science without certainties should be inadmissible. Since at trial the fact finder must

arrive at some degree of certainty as to the facts at issue, if a science has no certainties to offer, how can it assist the trier of fact and so how can it be admissible as expert evidence?

35. In footnote 215 at page 299, The Author takes a delightful, cultivated poke at the critics who repeatedly use their law journal articles to qualify in court as experts: "In an age that applauds 'recycling' as 'politically correct,' such multiple use of one's literary (if not scholarly) product is no doubt a laudable civic pursuit."

36. Footnote 220 at page 300 says: "Perhaps the majority of the participants in 'handwriting analysis' for character trait divination do so as an avocation, rather than as a profession." I have never seen any form of the word "divination" used by any writer in character handwriting analysis. Only adversaries of the discipline use it as a disparaging description meant to wrench from their reader feelings of revulsion. How has a word associated with God and the things of God come to be used as an insult? What warp in the human mind has taken a high compliment, "She is divine!" and twisted the same root word into an insult: "But she engages in divination"?

37. At page 308, we read concerning the critics: "The intentional concealment of such bias [of marketing themselves as anti-handwriting-expertise experts] for the sake of creating the appearance of scholarly equanimity and objectivity is indeed a serious defect that is antithetical to credible scholarship." Credit The Author with being up front about his biases as to which document examiners are to be paid your expert witness fees. Can one say the same for those to whom he turned for advice and guidance and who pushed their economic agenda onto him? He never mentions speaking with any expert outside the closed, narrow confines of the 300-500 experts whom he and themselves find solely acceptable. This is not objective journalism by The Author.[20] As for his preferred experts, their self-interested self-recommendation for which he acts as front man is hardly a reliable recommendation.

38. In footnote 260 at page 309, we read a quote from the critics: "No court has ever explicitly considered the field of document examination." That is both true and false; thus it is one of those falsehoods which are the best weapons against the truth. As support for my consulting work, I continually add to a database with more than 4,500 case citations related to document examination. In that regard the statement is false. Since no court ever considers the entire field of any recognized discipline, but only the very specific expertise applicable to the particular case under review, the statement is true.

39. At page 312, it is described how Professor Kam addressed issues which the critics "accurately stated had never been explored in earlier years in a scientific way." I have concerns about Professor Kam's work. Major among these are: One, does he uncritically accept some premises of the critics; and, two, does he ask if the critics

maintain such premises in accordance with their very own criteria? As to the latter point, for example, did the critics ever do a study to prove that they can criticize anyone, much less handwriting experts, in a reliable way superior to the common folk? If not, by their own alleged value system they ought to refrain from criticizing anyone until they do such a study which is statistically analyzed, peer reviewed, and duly published, then replicated by others.

As to the former point, like The Author, does not Professor Kam start with the purported two fundamental principles of handwriting identification that all persons write differently from each other and that each person writes differently each time the person writes? These will not aid in the identification of any two writings as being by any one writer; they will not even help prove that two different people wrote two different writings. They only establish that every writing is in some way different from every other writing, which everyone knew anyway. Only a fundamental principle concerning similarity can be basis for identifying two writings as being by the same person. Once we establish which features of similarity will permit us to identify two writings as being by the same person, then and only then are we able to establish what kinds of differences prove that two different people made two different writings.

Variation will help identify two writings as being by the same writer only if it is established that the kinds, patterns and combinations of variations are the same in the two different writings, and that these kinds, patterns and combinations of variations are individualistically unique.

40. The Author gives much discussion of the proper method for making a handwriting examination. It is not clearly delineated in any one place. I respectfully suggest that the following is a much preferred method to assure scientific standards.

(a) The problem is posed to the examiner clearly and precisely as a factual enquiry; ex. gr.: Did or did not X write this signature?

(b) The expert assesses skills, tools and materials required and available to do the work. If an essential is missing, the commission is declined or referred elsewhere.

(c) The disputed writing is assessed for its sufficiency as examination material. Can any deficiency be supplied?

(d) The exemplar material is assessed for its sufficiency as comparison material. Can any deficiency be supplied?

(e) What are the identifying notes of the disputed writing considered in and of itself?

(f) What are the identifying notes of the exemplar writing considered in and of itself?

(g) A comparison *of the two sets of notes* is made. Which notes indicate common authorship and which notes indicate different authorship? The Author says we must start at this point, figuring the differences *between the two writings*. As you can see, start here and much of the science, work and professional responsibility has already been skipped.

(h) Could indications of common authorship in and of themselves, without reference to other traits, suffice to prove common authorship?

(i) Could indications of different authorship in and of themselves, without reference to other traits, suffice to prove different authorship?

(j) Under either hypothesis of common or different authorship, is there a reasonable explanation for the contrary indications?

(k) Might other material be required to resolve the matter?

(l) Once an initial opinion of common authorship or not is arrived at, one or more reliability checks are run.

(m) What qualification of the opinion is required because of any limitations encountered at any point in the analysis? The qualification is expressed in standardized terminology.[21]

(n) When a scientifically viable opinion is formed, the appropriate report in the circumstances is prepared and rendered to the enquirer.

(o) Throughout the process, proper records are made and kept, and proper handling/safeguarding of documents is assured. The chain of custody is properly maintained.

(p) Further work is done as required, such as trial preparation.

41. The heading at page 317 speaks of a "vibrant exchange of information." Elsewhere the exchange is said to be limited to as few as 100 and intimated never to exceed 500. No information exchange within a closed loop is vibrant, being closed both to sharing with outsiders and to in-put from outsiders. Closed systems tend to stagnate. On the same page it says that all this professional information is readily available to researchers. Both statements elsewhere in the paper and the experience of myself and others belie that assertion. Then it is claimed that "literally thousands of impressive contributions to the periodic literature are being made each decade." Not so, if one is speaking about the handful of inhabitants in the castle keep of mainstream American document examination. The numbers are not there, and some contributions are impressive only to the easily impressed.

42. At page 318, the Questioned Document Article Database is discussed. Inquiry of one of the authors about obtaining it resulted in a curt refusal upon query concerning my professional associations. If one publishes, one publishes to the universe. The

deliberate refusal to disseminate scholarly work to all with a legitimate interest bears witness that one is not scholarly at all.

43. At page 321, we read of scientific method: "It is whether preconceived notions as to the desired outcome drive the examination, or whether there is a neutral methodology followed." It is commendable that The Author so proves that his own paper does not accord with scientific method.

44. At page 322 and elsewhere, the author makes a point that most examinations by handwriting experts result in exclusions; that is, saying who did not write something. Ought we to expect less from the least of forensic disciplines? (See paragraph 29 above.) In an anonymous note case, by proper procedure an expert asks for exemplars from all persons associated with the event. If ten writers are represented in the pool of exemplars, at least nine of them will be eliminated. Also, once the maker of a forgery is identified, more than 5,000,000,000 others in the world are eliminated. The numbers game on exclusions sounds good until we evaluate it.

45. The case *United States of America; Government of the Virgin Islands v Velasquez*, 64 F2 844, 33 V.Is. 265 (3 Cir 1995) is noted several times. Lynn Bonjour did superb work in it. She perished in an automobile accident. If it has not occurred, there ought to be some memorial dedicated to her for her quality contributions. Who could read this case and not realize that indeed, when done properly as she did it, handwriting identification is a scientific endeavor?

46. The discussion returns to the ear expert and engages in other more or less inconsequential digressions. One gratuitous aside at page 325 is the unjustified, and hopefully unintentional, insult to Steven Slyter's personal integrity. I would not myself rely on his book, yet he deserves respect for the courage to contribute. Is discussion of Slyter's book part of using graphologists as scape-goats for the ills of document examination? What would The Author and his confreres do for an excuse if there were no graphologists? I suspect they would turn on each other and invent an outcast class from among their own numbers. Our profession will not be mature until it can recognize and correct its own inadequacies without the need to blame someone else.

47. At page 326 The Author tells us that most questioned document examiners are eminently qualified. Is he relenting and allowing that some practitioners beyond his outside limit of 500 acceptable ones are qualified? Not at all, because at the beginning of his paper he arbitrarily limited the title "questioned document examiner." He is finally admitting explicitly that there may be an odd child or two within the family of his recognized document examination organizations.

48. At page 327, there is discussion of "hard" and "soft" sciences. It is, I suspect, a way to layer scientists into the upper crust of "pure" mathematicians, next that of "hard"

sciences with full blown mathematical applications, then a mid layer of "hard" science but less math, next down "soft" science and some math, lower yet "soft" and no math worthy of the name. Then below everything the great mass of the unmathematical and unscientific, with those labeled "pseudo-scientists" being the lowest of the low. The last is the ultimate insult which can be given to any group, never mind that their taxes and citizen support are footing the bill and maintaining the honorifics for the scientific upper crust.

49. At page 330, Dean Berger is quoted as to four judicial concerns about expert testimony being admissible. He should be informed that he forgot maybe the most critical one: Will the expert evidence be of assistance to the fact finder?

50. At page 335 are given dismal numbers for the effectiveness, and maybe the honesty, of ABFDE certification. Starting in the 1970's, they gave away at least 180. The numbers which The Author provides show that they granted about a hundred certificates on purportedly proven merit. That is an average of approximately five a year. With 168 active Diplomates in 1997, they retrogressed from the 180-190 active at the beginning. Was your Federal tax money, which set up this little private QDE club, effectively or ineffectively invested?

51. At page 336, The Author is mistaken that the core handwriting group in American Board of Forensic Examiners (ABFE) all claim board certification and that they are also members of National Association of Document Examiners (NADE). Both assertions are false. Not all claim the certification and not all belong to NADE. Many in NADE initially supported ABFE because the propaganda outlined a forensic promised land. Unfortunately, that particular Moses wandered back out of the desert into another Egypt. Why did not The Author do balanced research and ascertain the number of members from AAFS and related organizations who took ABFE certification? I did a bit of it, but the requirement of earning a living prevents further investigation.

52. In footnote 367 at page 337, the journal citation is inaccurate. It should be 20 *Scientific Sleuthing* 12 (Winter 1996). The author of a letter to the editor was complaining because the editor noted the reality of an organization, Association of Forensic Document Examiners (AFDE), of whose existence she disapproved. The footnote erroneously connects AFDE to the American Board of Forensic Examiners (ABFE). AFDE is in no way similar to or associated with ABFE. I can think of no organization which is similar to ABFE. I attended their first conference when they were called American Board of Forensic Handwriting Analysts. It was such an experience that the first thing I did upon returning home was to go to my computer and wipe their name off all my records. I will share one of many reasons why.

During one meeting, a fellow stood up and explained how he handled the problem of having cross-examiners obtain his notes and ask embarrassing questions based on them. He rewrote them after forming his final opinion, destroyed the original and testified that the revision was the original. At the break I asked the leaders to take a strong stand against such practices. One of them told the group that "we" do not approve of such practices, "but you know what works best in your jurisdiction." Later, the man who rewrote his notes was made their ethics chairman.

ENDNOTES:

1. D. Michael Risinger, Mark P. Denbeaux, and Michael J. Saks. *Exorcism of Ignorance as a Proxy for Rational Knowledge; the Lessons of Handwriting Identification Expertise.* UNIVERSITY OF PENNSYLVANIA LAW REVIEW, 731-92 (Jan. 1989)

2. Roy J. Deferrari. *A Latin-English Dictionary of St. Thomas Aquinas*, Boston, St. Paul Editions, 1960.

"Science" comes from the Latin "scire," "to know." If not a special kind of assured knowledge, how can a science assist the trier of fact to know a specialized fact?

3. For two of several classical discussions on whether handwriting is scientific, see:

M. K. Mehta. *Whether the Identification of Handwriting is a Science.* 36 ALL INDIA REPORTER. 14+ (Feb.-March 1949).

Ordway Hilton. *Science and the Scientific Examination of Signatures.* 24 TULARE LAW REVIEW. 204-10 (Dec. 1949).

4. Marcel B. Matley. *Forensic Handwriting Identification: Is it Legally a Science? A Review of Court Cases Which Hold Handwriting Examination to be a Science.* 3 INTERNATIONAL JOURNAL OF FORENSIC DOCUMENT EXAMINERS, 105-13 (April-June 1997).

5. L. Michel and Peter E. Baier. *Diaries of Adolf Hitler. Implication for Document Examiners.* 25 FORENSIC SCIENCE SOCIETY JOURNAL, 167-78 (1985).

The abstract reads in part: "Material purporting to be from the diaries of Adolf Hitler was authenticated by three different document examiners before tests of the paper and handwriting by other experts proved the 'diaries' to be forgeries. . . . [E]rrors arose from . . . using poor comparison material [and] making insufficient examination of the conditions of origins of the questioned material. . . . " I do not think the examiners who

made the initial, but honest, error ought to be shamed; shame belongs to the many who claim to have solved the problem, when it was more a matter of being interviewed by news media after the dust had settled from the real battle.

6. Royal Canadian Mounted Police. *Proceedings of Seminar no. 5: Questioned Documents in Crime Detection.* Ottawa, Canada, RCMP, Oct. 27-Nov. 1, 1958. Page 131.

7. Sandra L. Ramsey. *Effect of Computers on Forensic Document Examiners.* American Academy of Forensic Sciences, Proceedings, Vol. 1, Annual Meeting, Seattle, WA, February 13-18, 1995.

8. 379 NW2 70 (MN 1985). At page 79: "The admissibility of a graphological personality assessment is a matter of first impression in this state. . . . [A] graphological personality assessment does not meet the standard required for admission of expert testimony under *Frye* and its Minnesota progeny." However, there are cases which have ruled that a physician, mental expert (what we now call a psychologist) or handwriting expert may qualify to testify as to mental condition indicated by handwriting. For those interested in researching this topic, begin with Albert S. Osborn, *Questioned Documents, second edition*, reprinted by Nelson-Hall of Chicago. See Section 2-14 of Part Two, at page 751: "Mental Condition Indicated by Handwriting." In these and later cases, the expert witness *was not* a graphologist.

9. Only the Questioned Document Section of the Academy is referred to throughout, though I will be saying "American Academy of Forensic Sciences."

10. Marcel B. Matley. *The Graphologist as Expert Witness; a Survey of Some Reported Cases*. 17 NATIONAL ASSOCIATION OF DOCUMENT EXAMINERS JOURNAL, 15-27 (Spring 1995).

11. Even Albert S. Osborn had cases where the trial court rejected his opinion, and apparently a bit roughly. In the case *Succession of Roth,* 133 S2 150, 86 ALR2 1278 (LA App 1961), Osborn put in a claim for fees of $24,250.00, which the trial court reduced to $6,250, the reduction being upheld on appeal. Would anyone argue that that diminishes Osborn's status? In *Wright v Simpson,* 232 KY 148, 22 SW2 583 (1929), Osborn's opinion did not prevail over attesting witnesses and experts on the opposing side. Neither Samson nor David won all his battles.

12. In essence, does it come down to not much more than not yet having died? If one was gifted with the original unearned certification, one need not meet subsequent stricter requirements (ex. gr., earning a college degree) which serve to keep the numbers, and thus the competition, down, a control of the trade to which The Author unwittingly and inadvertently testifies.

13. Marcel B. Matley. *Studies in Questioned Documents, Number Two: Exorcism of Ignorance; a Reply from a Handwriting Expert.* San Francisco, CA, Handwriting Services of California, 1990.

14. Marcel B. Matley. *Studies in Questioned Documents, Number Five: Health and Handwriting; an Annotated Bibliography of Forensic Periodical Literature. Second edition, enlarged.* San Francisco, CA, Handwriting Services of California, 1994.

15. Originally published in 1990 as *Witnessing to the Truth of Documents; An Index to Periodical Literature in English on Document Examination, Handwriting Expertise and Expert Testimony*, Marcel B. Matley, compiler. It was favorably reviewed by Richard N. Totty in *Forensic Science Society Journal* and by others elsewhere. The current edition is scheduled to be issued by Shunderson Communications of Ottawa, Canada, publishers of the prestigious *International Journal of Forensic Document Examiners.*

16. For a summary of his ideas, see "Popper, Karl Raimond," pages 398-401, Volume 6, *The Encyclopedia of Philosophy*, Paul Edwards, Editor in Chief. New York, Macmillan Publishing Co., 1967.

17. Charles Hardless, Jr., in *The Identification of Handwriting and the Detection of Forgery,* Calcutta, Hardless Press, 1912, says at page 14: "The lay method of comparing handwritings as to their identity or otherwise consists of a comparison by formation . . . noting the similarities and dissimilarities in the particular make of letters of the same significance." At page 28 he describes the new way: "The scientific and more effective method . . . is by judging the personal or individual writing habits . . . . [N]o two persons manipulate their pens exactly alike and hence in no two persons is the writing habit alike, even in cases where they form their letters alike." At page 29 he completes the general description of the scientific method: "[C]areful scrutiny of such details as the movement, presentation and pressure of the pen, the degree of co-ordination between strokes and curves of the writing will show differences, thereby distinguishing each writer."

18. Robert Saudek. *Experiments with Handwriting.* London, George Allen & Unwin, 1929. Reprint: Sacramento, CA, Books for Professionals, 1978.

19. W. A. Schulenberger. *Anonymous Letters: Some Principles Affecting the Search for and Identification of Writers of Threatening or Offensive Letters.*

Information taken from Saudek's *Experiments with Handwriting* without citation and given in simplified form includes: Signs of Speed and Slowness, Primary Signs of Unnaturalness, Mechanical and Physiological Principles of Handwriting, and General Scale of Difficulty of Altering Writing Elements.

20. Robert E. Backman, Curator of Handwriting Analysis Research Library, Greenfield, MA, supplied the following comments:

"After carefully studying Andre A. Moenssens' lengthy article, and having reviewed Marcel B. Matley's rebuttal, these thoughts occurred to me:

"On page 308, in footnote 253, Prof. Moenssens 'discloses that his studies in criminalistics began in Belgium in the early 1950's under the tutelage of the late Major (ret.) Georges H. Defawe, a noted court expert in questioned document examinations. During the mentoring process, the author was required to work on document examinations under the supervision of Major Defawe, but after two years of such study chose to specialize in another specialty area of the forensic sciences. Despite the author's study and practical experience, and despite the fact that he has been asked to do so on many occasions, he has not testified as a document expert.'

"COMMENT: Major Defawe was in the Belgium National Crime Laboratory in Brussels. He was also conversant with French, German, Swiss and Austrian police methods as well as with Lombroso's methods in Italy.

"Further, the Major was also conversant with the graphology practiced in those countries, and he held that a knowledge of graphology sharpened the examiner's ability to observe tiny details of handwriting that ordinary laymen were entirely unaware of.

"It is interesting that the Professor omitted this information in his lengthy paper. It appears that the Professor has never held himself out as a questioned document examiner. However, his two years supervised tutelage under the Major apparently 'qualified' him to write this lengthy critique. It seems that the Professor also has his biases."

21. McAlexander, Thomas V.; Beck, Jan; and Dick, Ronald M. *Standardization of Handwriting Opinion Terminology.* (Letter to editor) 36 JOURNAL OF FORENSIC SCIENCES, 257-60 (Jan. 1991).

My criticism of this witness' testimony is that there is too much learning in it, and it does not present the matter as it appears to a casual observer. In the case of Strode v. Strode, supra, the court said: "No one can make the comparison [i. e., between the disputed and the admitted signatures] without concluding that the general appearance of the writings is the same, and a casual observer would no doubt unhesitatingly pronounce them as executed by the same person." The expert did not undertake to present the general appearance of things—just how the matter strikes one like myself, holding the disputed and genuine together, to determine whether they are or are not substantially alike. He presented it as things appeared to him after days of study and the use of the instruments which experts avail themselves of in reaching their conclusions. All the results are mixed up together, without any separation of the important from the unimportant. This has made it an extremely difficult job to master it all; so difficult that I have not undertaken to do this. I did not have the patience to do this, and I did not think that the case called for such patience.

[W]hat the expert has to say on these subjects is buried in a mass of detail, and, as stated, I have not had the patience to sit up with it and dig it out.... As to the testimony of the five bankers, relied on by the claimants, it is greatly weakened by the consideration that not one of them gave a reason for the faith that was in him. They simply expressed the bald opinion that the signatures were genuine. These witnesses may be termed business experts, as distinguished from professional experts.

Wigmore would seem to think that an expert's opinion, without the reasoning on which it is based, is of but little value.... I was entitled to have the basis of the opinions of those bankers, in order that it might test their soundness, and this has not been afforded me. I never like to follow any one blindly.

In re Varney (two cases), 22 Fed2 230, at 236 et seq. (Dist. Ct., E. D. KY 1927)

EPILOGUE:
PRETENSIONS ARE AS PREVALENT
AS THEY ARE PERENNIAL

Socrates devoted his teaching life to refuting the methods of the sophists. The ancient sophists did not contribute to the pursuit of knowledge, nor did they demonstrate wisdom. They devoted themselves to showing how no one was wise or could be sure of anything. They created logical games to prove their point, namely, that there was no point to the pursuit of truth. Yet sophists laid claim to the title "philosopher," "the lover of wisdom." Modern day handwriting sophists have laid claim to the title "scientist," "the one who knows." All they claim to know is that nothing is known for sure, except for one thing: They alone know how someone someday might know something, and they will be that someone who will then teach the rest of us. Yet, they must have given us some good reasons why we should continue funding their wages, benefits, tenure, laboratories, research, travels to conferences, and, through the ever increasing costs of our litigations, to fund also the outside work they pursue as scientific expert witnesses

The burden of their theory is that nothing has been known since the beginning of time about any topic they are the experts in, that no one but they know anything today, and that no one in the future will know anything until they have finally discovered it. The sophistries they employ to gain credence for their views have been fairly well exposed in this monograph and in replies from others whose works I have cited. By so disagreeing with them, all their critics have merited to be served a sophistical hemlock, just as Socrates was served a cup of the real stuff. Note how in *Starzecpyzel* and in their *Science and Nonscience* paper they "executed" from their Land of Science all those who had a differing view, asserting that those whom they disagree with are utterly non-scientific. Is it more than just rumor that, in their world of sophistical academia, students and colleagues who stray from the shared myths are served an academic hemlock: degrees denied, reputations destroyed, access to grants cut off, submissions to peer reviewed publications peer reviewed into oblivion, teaching appointments withheld?

I say this to make very clear that the battle against sophistry will end only when Gabriel blows the final trumpet. To be free of sophistry is to be free of intellectual tyranny, and like all freedom from tyranny the price is eternal vigilance and eternal maintenance of the weapons of freedom. In all such cases, the tyranny and the attacks of sophistry are far easier to put forth by its partisans and far easier to succumb to by the minds they target than the reply is. Both to put forth and to accept a reply to sophistry necessarily entail a double difficulty: First, to expose the fallacies of sophistry and, second, to explain the reasons for the truth. Since sophistry by definition is false and superficial use of one's reasoning ability (that is, not to bother with too much reasoning),

the logic which is its antidote requires disciplined study of its rules, their careful and consistent application to practical problems, and the work of verification of one's premises. With sophistry, one can know all there is to know without knowing anything except one's self-assured superiority to others. The description of the defense experts' testimony in Federal Supplemental Reporter for *Starzecpyzel* is laced with such self-assurance, while the transcript of the *Daubert* hearing itself reeks of it.

It is in that regard that Mary Wenderoth Kelly is shown a far superior human being to her counterparts for the defense, for she always spoke with modesty and earnestness, always honestly realizing her limitations, always recognizing there were materials to be had which she did not have at hand, conceding there was more to be known than she knew. She was thus the finest expert of the three, for she alone did not know it all, she alone had no recourse to sophistry, she alone had no put-down for another, even for the two who claimed no knowledge of handwriting except for their conviction that all others were ignorant. Though I have had very pointed things to say regarding her testimony as to theory and method, I would wish to develop the fineness of character which she showed. That kind of character gives to our profession a promise with which very few professions might be blessed. It shall surely assure our profession's survival long after the anti-expert experts have gone into the oblivion they so justly earned.

In the meantime, I respectfully submit that we build our future on the excellent foundations our forebears in this profession have laid down for us. The genuine scientific research which will prove our science to the courts and legal profession is that which has proved it before. For a start, we need only study the researches which were done over the past hundred plus years and duplicate them with modern tools. Instead of motion picture film, we can use the cam-corder. Instead of laborious manual calculations, we can use computer software. Instead of the old nib pen and papers, we can use the rich assortment of modern pens and papers. As to the reality of handwriting, at its core it is neither new nor old; because, when the first human beings wrote in the earth where the berries were to be gathered or what was the plan to hunt the prey, they used the exact same anatomical parts and physiological processes that you must use when you next pick up pen and paper to write.

In that way alone will the pretensions, which chicanery, ignorance and sophistry forever throw in the pathway of proper evidence against forgery and forgers, be exposed and chased from courts of law. In that way alone will the false experts among us also be exposed and either forced into learning proper theory and technique or made to fade into the shadows of their own shame.

APPENDIX A:
A FURTHER NOTE ON MOENSSENS' PAPER

Almost 18 months after I had written my reply to Professor Moenssens' paper which appeared in *UMKC Law Review*, I received an outline of his presentation given at the 1972 conference of International Graphoanalysis Society. It illustrates how perception can be very subjective. In footnote 40, page 264, of his paper, the Professor discusses how graphoanalysts were claiming to have received instructions in questioned document examination from him and what his reaction was. I quote the footnote in full:

"This author experienced a similar predicament. After having been asked to address two annual meetings of IGAS in the early 1970's, he discussed there evidential principles and also remarked on the difference between questioned document examination and Graphoanalysis. He stressed that the listeners were not competent to qualify as document experts on the strength of their IGAS training (a premise with which the management of IGAS was in full accord). It was reported to the author later, however, that several of the graphoanalysts who had attended his lectures represented to courts in sworn testimony that they had been 'instructed in questioned document examination' by Professor Moenssens. Needless to say, no such 'instruction' had ever occurred! When at the last one of his appearances before IGAS, the author stated on the public forum that he had been informed of this practice and believed such testimony to be at least 'intellectually dishonest' and probably also perjury, invitations to address IGAS meetings understandably ceased!"

The outline of his 1972 presentation at IGAS covers Monday through Thursday. Monday through Wednesday all begin with introductory remarks. Monday's topics are admissibility of questioned document evidence, including case histories, and demonstrations in the courtroom, both experiments and court exhibits. Tuesday's topics are constitutional limitations, qualifications of the expert, the expert's conclusions and statements of ultimate fact, and disagreement among experts. Wednesday's topics are the new expert, Graphoanalysis and the courts, and four aspects of ethics. Thursday offers a summary and general discussion followed by an examination. If all that is not instruction in questioned documents, then I am at a loss to imagine what possibly could be instruction. Maybe the author offered no information, in which case there would have been no instruction, but then he would have failed to earn the honorarium paid him, which my informant said was substantial for the early 1970's.

If four full days of information on all those topics offered by a law professor and culminating with an examination to ascertain that attendees absorbed the information is not instruction, then is a five-day questioned documents course offered by the FBI at its facilities in Quantico, VA, to be considered instruction? Does an extra day's presentation

make all the difference? Would the Professor assert that all his new found questioned document friends, who testify under oath that the five-day FBI course is part of the training and instruction qualifying them as experts, are being intellectually dishonest and probably perjurious? When a public agency or a private organization offers a one-day program on IR-UV photography or ESDA or paper manufacturing or any other topic in questioned documents, are those, who attend and put such attendance on their curriculum vitae as instruction and training, being four times as intellectually dishonest as the Professor's students who did so for a full four-day program? Will the Professor tell the American Academy of Forensic Sciences (AAFS) document experts, when next he speaks to them, that he has determined that any program of four or less days duration is not instruction and must not be said so to be when under oath? Will he preface all further presentations he gives at professional meetings with the assertion that, since he is talking for four days of less, no one is to claim that he has offered any instruction of any kind?

When I heard him speak at the year 2000 Conference of AAFS, I thought his talk was very instructive. I wonder if he thought so too? If so, then the IGAS people who attended his 1972 four-day program would be intellectually dishonest not to state that they had received instruction from him when testifying to their training and education in questioned documents.

APPENDIX B:
AFFIDAVITS IN *CUSACK* CASE

There are two *Cusack* cases. They are *United States vs. Cusack*, Case No. 98 Cr. 691 (DLC) (S.D. NY 1999), and *Cusack, et al., against 60 Minutes Division of CBS, et al.* (Supreme Court, County of New York, Index No. 600060/98). The first will be referred to as the Federal criminal case and the second as the New York civil case, or simply as the criminal and civil cases respectively.

Beginning on the next page are the two affidavits which I submitted in the New York State civil case. Defendants had filed a motion for dismissal based on the fact that Lex Cusack had been convicted in Federal criminal court and sentenced. In reply Plaintiffs filed affidavits by three handwriting experts answering the testimony of the two government handwriting experts in the Federal criminal case. This was necessary in order to show that such testimony would not stand up against proper challenge and contrary expert testimony and that defense attorney in the criminal trial had failed to bring forth substantial evidence available for the defense as well as to conduct proper cross-examination with questions and learned treatises made available to defense counsel prior to the trial. Further, several persons would have made themselves available as defense witnesses in the criminal case, but they were never called. These all submitted affidavits in the civil case to demonstrate that substantial, relevant factual issues had not been properly litigated, much less raised, during the criminal trial. Many other legal and factual issues were raised by Plaintiffs in reply to Defendants' motion for dismissal in the civil case.

Defense reply to Plaintiff's reply to motion for dismissal was merely by one Defendant's reassertion that Cusack had been convicted in Federal Court. Since no factual issue raised in Plaintiffs' reply had been specifically addressed by the reply, there was then raised a legal issue as to whether such failure to address specifically any such factual issue was by law an admission by Defendants of all of Plaintiffs' factual assertions. As of the time of this writing, the Court had made no ruling on Defendants' motion for dismissal and Plaintiffs' counter motions in the civil case. The intended appeal and motion for a new trial have yet to be effectuated in the criminal case.

The above is given only as context for the following facsimile reproductions of my affidavits and to show that they were a small portion of the evidential issues raised by the several persons submitting affidavits for Plaintiffs in the civil case. The exhibit common to both affidavits is not reproduced for the second. Exhibits which copied the published work of another are not reproduced here. The references in the body of the affidavit will allow retrieval of such published matter.

I, Marcel B. Matley, swear and affirm:

1. I am an Examiner of Questioned Documents and have been so employed since 1985. My business office is located at 3092 Army St., San Francisco, CA. I have been certified as an expert witness in the field of questioned documents in Federal Bankruptcy Court, California Municipal and Superior Courts, in Arbitration and Administrative Hearings in the State of California, and in Probate Court in the State of Texas, and I have testified regarding documents in each of these areas. A copy of my current Curriculum Vitae and General Resume, outlining my training, experience, qualifications and professional activities, is attached hereto as Exhibit A and incorporated herein as if set forth in full.

2. I was asked to evaluate the transcript of the testimony of Duayne J. Dillon (hereinafter: "Dillon"), given in the case *United States of America vs. Lawrence X. Cusack, Jr.*, United States District Court, Southern District of New York, Case No. 98 Cr. 691 (DLC). I was asked to evaluate it in terms of its validity and reliability in light of the standard and accepted theory and methods in the field of questioned documents. In summary, my evaluations are:

(a) Dillon's theoretical statements were contrary to accepted theory in the field of questioned documents for the identification of handwriting and signatures;

(b) Dillon's methodology was incorrect and at variance with the methodology recommended by recognized authorities

in the field of questioned documents;

(c) Dillon's observations were not adequate to support his conclusions based on such observations;

(d) Dillon failed to take into account the guidance which other disciplines provide to questioned documents regarding handwriting, particularly the field of medicine which has published research and case reports regarding the effects of health and medications on handwriting; and

(e) Much of Dillon's logic violated proper rules of reasoning.

3. At 1700:8-13 Dillon said: "Well, handwriting identification, which is the primary activity of forensic document examiners, is based upon the proposition that each individual has an individualized style of writing which contains within it characteristics which are observable, comparable, and can lead to an opinion as to the authenticity of the writing." That is patently wrong.

First, any given handwriting is only identifiable *if it contains* individualized characteristics. The expert must first prove that *this* questioned writing *does* in fact have sufficient individualized characteristics to be identifiable. With the above quote Dillon simply absolved himself from having to prove the first two things he must prove in order to give a correct opinion as to handwriting identification, namely: (1) *what* are the individual and uniquely identifying characteristics of *this* questioned writing and (2) are they sufficient in number and quality to tie *this* questioned writing to *one and only one* potential

writer.

Second, Dillon missed the scientific reasons why any handwriting can have uniquely identifying characteristics tieing it to the person who wrote it and to no other person as its writer. There are twelve causes of individuality in handwriting, which are listed in Exhibit B, copied from Robert Saudek's *Experiments with Handwriting*, pages 234-235. We cannot presume that any one of them operates in any given case until we prove that it does. Dillon never *proved* that any of the twelve causes of individuality in handwriting operates in the particular writings he examined in this case. He *presumed* that some do but he never demonstrated the dynamics of any of them *precisely as* causes of individuality in this case.

4. At 1700:14-16, Dillon was asked: "What are the kind of features in a person's writing that allow you to make an identification or determine authenticity?" He never answered that question, but instead talked about class and individual characteristics and of line quality. At 1701:10-16 he spoke of deviations from copybook style which one was taught as "personalizing characteristics or features," but he only assumed it operates in this case, nor did he say what are the *kinds of features* that permit identification. He concluded the same passage with: "We also look for those features and a combination of those features which are directly related to the manner in which the pen is controlled by the writer." Dillon never satisfied *his own* criteria for each writer whom he considered, namely,

Defendant, John F. Kennedy (hereinafter: "JFK") and Robert F. Kennedy. To do so he must first establish for each writer:

(a) The exact copybook style such writer was first taught;

(b) The features in such writer's style during the period of time for the questioned writings which were deviations from such copybook style;

(c) The precise manner in which such writer controlled the pen during the period of time in which the questioned writings were made;

(d) The features in the writings of such person which are the direct effect of such manner of controlling the pen;

(e) The dynamics which explain why such features are the effects of such manner of holding the pen; and

(f) How such features relate as independently occurring features so that they make for a unique combination of identifying traits.

Not having done what *he said* must be done, Dillon's opinion is without *any* valid theoretical foundation.

5. At 1703:19-21 Dillon made this theoretical statement: "An individual has a conservation of energy when they are writing. A person typically uses the least amount of energy that is necessary to execute the signature." I know of no published research or other authority to support that statement. But be that as it may, Exhibit C shows writings in question in this case which were written with very little energy, such that the pen barely touched the

paper in places. Exhibit C comprises microphotographs taken by Robert J. Phillips (hereinafter: "Phillips"). In these particular samples a fluid ink pen was used, thus allowing a visible ductus with minimal penpoint pressure.

Continuing the same answer from 1703:19-21, Dillon stated at 1704:1-17 that a forger in imitating another's writing leaves certain traits, such as slow speed, stops and starts which are not "normal," blunt endings, tremor, awkwardness, and patching. These are some of what handwriting experts term *indicia of forgery*. However, Dillon fails to note that each of these occur for other reasons, thus their mere presence proves nothing as to an imitation forgery. One has to do the following to make such proof:

(a) List all such features in the questioned writing;

(b) Observe the *quality* of such features, as they differ in quality in accordance with their causes;

(c) Determine the *cause* of each such feature, whether it be from mechanical problems, health impediment, unusual writing situation, or several other possibilities;

(d) For each such feature, see if it occurs in the usual and customary writings of the purported author;

(e) Determine the *quality* and *cause* of each such feature which might be found in the usual and customary writings of the purported author.

Then and only then may one make a comparison and develop an opinion. Dillon did not testify to any of that work, nor did defense attorney tax him with the omission.

6. At 1706:11-15 Dillon mentioned illness as one of the causes of changes in an individual's writing. In the words of a car rental ad, that is "not exactly." Illness *can* cause a change in writing but *only* at its onset. If the illness is acute, its effect on handwriting ceases when the cause ceases. If it be chronic, the effect perdures *unless* the individual copes successfully when writing *or* there is remission. It is common and public knowledge that JFK had severe health problems. What is required of any competent, conscientious and thorough expert *prior* to completing an examination of JFK's writing is to obtain credible medical reports of his illnesses and therapies and to correlate these chronologically with the writings under examination. To anyone with a rudimentary knowledge of the effects of health factors on handwriting, it is obvious that the questioned writings of JFK are characterized by such effects. In Exhibit C the lightness of the pen on the paper is one such effect, the ataxia shown at the start of some strokes is another. There are several others throughout the 129 microphotographs which Phillips sent to me for study. The failure to take all of the above into consideration is another reason why Dillon's opinion is not reliable.

Consideration of the writer's health has been routine in forensic handwriting identification since before Ordway Hilton penned his paper "Consideration of the writer's health in identifying signatures and detecting forgery," *14 Journal of Forensic Sciences*, April 1969, pages 157-66.

7. At 1707:9-17 a portion of the documents which Dillon

examined at the Kennedy Library were offered into evidence, and defense attorney said "No objection." The defense must see absolutely every writing Dillon examined. If one single writing contradicts the observations he testified to, he can be impeached. At 1708:6-11 the evidence is that the same thing happened for documents provided to Dillon by the Government. Dillon stated that he "scanned" much of the material and did not use magnification on all of it. Since his use of magnification was mentioned to enhance his opinion's reliability, his failure to use it on all relevant writings undermines his reliability, because he can never know whether or not he missed data disproving his conclusions. Additionally, he never gave the criteria for selecting what to examine closely or what to bring into court, nor did defense counsel examine him on those criteria.

8. At 1710:20-1711:4 Dillon explains that one must have a model in order to imitate another person's handwriting. Defense counsel did not force the Government to prove that whoever allegedly forged the writings had sufficient models of JFK's writing. If a necessary link in the expert evidence is not supplied, the chain of proof is incomplete and the conclusion cannot follow from the premises. Defense counsel did not pursue this missing link.

9. At 1725:18-1726:1 Dillon asserted that fast writing leaves tapered endings and slow writing leaves blunt endings. Once again, "not exactly." It depends on the nature of the pen as well as on the *kind of speed* a fast

writer has, but I will only discuss the problem of pens. See Exhibit C where some of the writings have what appear to be blunt endings which are darker colored than the rest of the stroke. The cause is fluid ink flow back. When a pen uses fluid (or aqueous) ink, meaning water based and liquid, during the act of writing, the penpoint has a little droplet of ink between it and the paper. When the pen is picked up, that droplet of ink remains on the paper and flows back along the wet ink line. The line thus appears darker towards the end and appears blunt because the droplet rounds off the end of the stroke before flowing back. This is rudimentary information which any handwriting expert should know of and consider.

For example, Ordway Hilton, in "Effects of writing instruments on handwriting details," *29 Journal of Forensic Sciences*, Jan. 1984, pages 80-6, illustrates in Figure 1 at page 81 writings made by the same person with different instruments, which writings have some endings which are blunt, some tapered, and some with fluid ink flow back. Defense attorney did not challenge Dillon on this omission and as to whether it be from unawareness or from calculation.

The Hilton paper cited above also refutes Dillon's assertion at 1788:15 *et seq.* that different writing instruments make little or no difference in one's writing traits. Indeed, Dillon's failure to address the nature of the pens used in the writings which he examined invalidates his opinions. Exhibit E shows enlargements of figures from

Hilton's paper.

10. At 1729:23-1730:10 Dillon testified that the word "acknowledged" was microphotographed through a microscope. Even in pica type the word is 1.1 inches long. Using 0.7 power, the smallest setting on my Nova STZ microscope, the field of vision is barely an inch. Any degree of magnification would have less than one inch in the field of view, not enough to take a photo of the entire handwritten word "acknowledged." Exhibit D shows one of the questioned writings. With that style of writing, a handwritten word of 11 letters would have to be about two inches long. Defense attorney did not challenge Dillon on this technical error in his testimony.

11. Throughout his testimony Dillon showed enlargements of the questioned writings and claimed that certain features show it to be slow and that others show it to be an imitation. Defense attorney did not challenge the unstated and unproven assumptions which underlie Dillon's claims. The following are examples and all are false assumptions:

(a) Only forgery slows the questioned writing; if Dillon had not assumed this, he would have investigated other potential causes of slow writing;

(b) The writings which he did not examine closely would not have contrary indications; if Dillon had not assumed this, he would have examined *all* submitted writings equally;

(c) Anything resembling any of the *indicia of forgery* must be one; if Dillon had not assumed this, he would have considered other causes for similar phenomena;

(d) The *indicia of forgery* are proof of forgery; if Dillon had not assumed this, he would have seen if any authentic writing had any such *indicia* and would have considered alternative causes for them; and

(e) No writing is an amalgam of speed and slowness; if Dillon had not assumed this, he would have, one, delineated all signs of speed and slowness in each writing, two, related the signs of speed to the signs of slowness, three, evaluated which set of signs dominates and, four, determined the cause for each of the contrary signs.

Exhibit D has signs of slow writing, but the signs of fast writing dominate, such as primary and secondary width of the writing, left margin increasing to the right, increasing continuity, simplification of forms, tendency to threading, and others. The contrary signs can only be explained by a physical impairment in the motor sequence. Note that Exhibit D was written with fluid ink and thus has endings which appear to be blunt. As Exhibit C shows, in places the ink runs into the fibers of the paper, which is typical of fluid ink on unsized or poorly sized paper. Dillon's testimony did not cover these factors, and the defense attorney did not challenge him on them.

12. Throughout Dillon's testimony the prosecutor asked leading questions, questions containing a testimonial statement by the prosecutor and telling Dillon the answer desired. For example, at 1733:15-17 is this question: "Let's take a look now at Government's Exhibit 221E. This is a look at some of the line quality in Government's

Exhibit 1023, is that correct?" Dillon agreed with the testimony which the prosecutor had just given and asked Dillon to agree to. Whenever that occurred, the defense attorney did not object.

13. At 1734:4-10 Dillon spoke of an "awkward-appearing angular change of direction." Exhibit D shows such features, but only where finger movement alone is required. Where hand and/or full arm movement dominates, such angular awkwardness does not occur. That is a tell-tale sign that the writer has graphic ataxia affecting the fine neuro-muscular movements in handwriting, those movements involving the fingers. Since Dillon insists that fast writing is genuine and that slow writing is forgery, he must logically say that Exhibit D is genuine whenever it shows only hand and/or arm movement but that it is forged when it shows finger movement. This is just one example of the many logical contradictions which Dillon's theories lead to. Defense attorney never challenged such logical contradictions.

14. At 1741:13-22 Dillon claimed that there was less variation in the alleged forgeries because the imitator did not have enough material to copy. That is completely speculative and illogical. First, absence of a thing is not evidence of anything, so it is improper to base a scientific opinion on the absence of evidence. In order to base any conclusion on the presence of variation, the expert must first determine the cause for each such variation. For example, research has proven that the same lower case letter

may vary in form depending on whether it is at the beginning or end or in the middle of a word, as well as to which letter is to its right or left. One such research report is by Alan M. Wing, et al., "The consistency of cursive letter formation as a function of position in the word," *54 Acta Psychologica*, 1983, pages 197-204. Defense attorney never challenged Dillon on any of his speculative statements.

15. The bulk of Dillon's comparative testimony focused on formation of letters, particularly capital letters. That is what was called "comparison by formation" or "comparison of hands" in the old days, and it was the kind of expert testimony which courts of law forbad under the English Common Law. The reason is simple: Such comparison by formation can "prove" any writing false and any writing genuine, and it can "prove" that almost any writer selected at random made a given disputed writing, as long as the writings stay within the same language and same nationality as in this case.

16. After comparing some capital letters, Dillon compared the word "of." As for the capital letters, Dillon considered any difference *between* questioned and exemplar writings as proof of different writers. At 1753:6-11 the prosecutor asked whether differences between words "of" *within* the questioned writings also prove different writers. Dillon said no, it showed the forgeries were done in groups, a reply which is a speculative and unfounded leap in logic. Defense counsel did not challenge this.

17. At 1767:2-10 Dillon testified that he made an

original discovery of secretarial writings among the materials supplied to him at Kennedy Library. Defense attorney did not ask Dillon the question that such discovery begs to be asked: "How do you know that the writings you claim are imitations of JFK are not by other secretaries to JFK?" Of course, Dillon could never know such a thing, and thus there could be no proof of forgery. In order to eliminate the reasonable possibility of the questioned writings being secretarial, Dillon must examine writings by every secretary whom JFK authorized to handwrite in his name and compare each of them to each and every questioned writing. There is no other scientific way to eliminate the reasonable hypothesis of secretarial authorship.

18. When Dillon stated that he determined some signatures submitted to him were secretarial, he necessarily implied that the exemplars which he did use were authenticated by himself as being genuine and not secretarial. Earlier at 1766:12-18 he also implied that he had authenticated his own JFK exemplars: "I was careful -- I tried to be careful in the manner in which *I selected* known signatures when in the Kennedy Library *rather than just accept* any document with a Kennedy signature on it as a valid known signature. I tried to focus on documents, but that by their very nature *would have likely been signed* by the President rather than the secretary." [Emphases added.] Defense attorney objected to none of these violations of the following guidelines for handwriting experts:

(a) An expert may not authenticate his own exemplars,

those genuine writings he compares to the disputed to determine authenticity in the latter. That is basing one's later opinion upon one's own former opinion, and thus it is building inference upon inference. This authentication of one's own exemplars is confirmed at 1796:14-24.

(b) Exemplar writings must be proved to the satisfaction of the judge to be genuine, while defense attorney did not require that the exemplar writings in this case be so proved;

(c) The words "*would have likely been signed*" indicate the lowest level of probability for genuineness, most insufficient to support a conclusion of falsity which meets all reasonable indications to the contrary.

19. At 1770:9-1771:9 Dillon gave an extended explanation why one of two receipts was traced from the other or both from a third. None of these reasons are supported by any research reported in the literature or by any published writings on traced forgeries. Defense attorney did not require authoritative support for any of Dillon's theories nor did he impeach with authoritative treatises to the contrary.

20. At 1779:22-1980:6 the prosecutor asked for an opinion regarding comparison of exhibits not before the court. Dillon answered the question without objection from defense counsel. When such evidence is not before the court, it is not subject to defense examination, nor available for jury deliberations, nor properly authenticated by the court before the expert testifies concerning same.

21. At 1785:16-1786:2 Dillon is asked if he examined zones of writing, and the reply was no, since graphologists do that to determine character. Yet Dillon examined capital letters, which are upper zone forms. He had a chart with "of," which in cursive writing goes into all three zones. Therefore, Dillon fails to understand the very simplistic term "zone," because he had done zonal analyses, all the while thinking he had not.

22. At 1797:17-22 there was this interchange on cross-examination:

"A. I had seen a sufficient amount of Kennedy writing for me to formulate the opinion that 35 of the documents that were introduced as Government exhibits were forgeries.

"Q. How much was that?

"A. It was as much as I could look at and compare in approximately six hours of work at the Kennedy Library."

However, he was not examined on the allocation of those six hours, or 360 minutes, to the various tasks he purportedly performed:

(a) How much time was spent in dealing with the staff, going to his assigned place and unpacking and setting up his equipment?

(b) How many boxes with how much material was given to Dillon and how much time did it take to look through the boxes to find likely comparison materials?

(c) How much time was consumed in identifying the alleged secretarial writings which had not been so identified before?

(d) How many comparison documents did he end up with and how long did it take to confirm their genuineness?

(e) Which documents did he take photos of and how much time did it take?

(f) He used a microscope and presumably his bare eyes. How much time was consumed in each of these two visual examinations?

(g) An expert must take contemporaneous notes of the identifying characteristics of his exemplar writings and, separately and independently of that, of the questioned writings. How much time was consumed in determining and recording these two sets of notes?

(h) Did he compare only the 35 Government documents which he said were forgeries, or did he compare all of them and found only 35 were forged? In either case, how much time did it take for each of the questioned documents individually?

(i) Did he have to attend to any personal needs, such as a drink of water? If so, how much time was consumed by such needs?

(j) How much time was needed to replace materials back in the boxes, give them to the staff and pack all his equipment?

(k) Did any person talk to Dillon during any of the above activities? If so, how much time was consumed?

360 minutes would permit barely more than ten minutes to photograph and examine under the microscope each of the 35 documents Dillon claimed were forged, *if he did nothing*

else. Doing the other necessary chores could possibly reduce time to less than a minute for each of the alleged forgeries, and that means no time would be left to say that the other Government documents were not forged.

23. Direct evidence of Dillon's superficial study of the JFK material is his failure to determine the proper reading of documents which he examined and on which he based his opinion prior to appearing in court. See at 1801:16 *et seq*. He either became an expert on JFK's writing or he did not. The passage demonstrates he did not.

24. At 1804:17-18 is another statement by Dillon evidencing the subjective and biased selection of documents: "I photographed those things which I thought would be helpful in the comparison process." Defense attorney did not ask for the criteria for such selection and the rationale for such criteria. The subjectivity and bias are shown by the subsequent admission that, if he had encountered certain names in the materials he examined, he "didn't see their significance at the particular time."

25. At 1810:4-8 defense attorney asked this: "We established on cross-examination that one of the possibilities was this was traced from an original real letter of John F. Kennedy's, and on redirect Mr. Neiman asked you isn't it also possible that whoever traced 501 traced it from a forgery, correct?" An objection was sustained, but defense counsel was wrong in asserting that any such thing had been established. As was already shown repeatedly, nothing that Dillon set forth as the theoretical

or factual bases for his opinions established such opinions.

26. That a critique of any particular point in Dillon's testimony is not included herein is not to be taken that such point does not merit critiquing. If every incorrect, incomplete, or inaccurate statement in the testimony were to be listed with the complete critique which it deserved, this declaration might well be ten times the length that it is.

I, Marcel B. Matley, swear and affirm under penalty of perjury under the applicable laws of United States of America that the foregoing is true and correct to the best of my knowledge and belief. This declaration was executed on this _____ Day of January, 2000, at San Francisco, California.

_____

Marcel B. Matley

A and M Matley — Handwriting Experts of California

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CURRICULUM VITAE AND GENERAL RESUME

MARCEL B. MATLEY

DOCUMENT EXAMINER AND HANDWRITING EXPERT

BORN: Phoenix, AZ, September 21, 1933.

SERVICES OFFERED:

- Handwriting authentication and signature verification, including numerals.
- Genuineness of logs, diaries, and other writings in series.
- Serve as either confidential consultant or testifying expert.
- Oral or written opinion, which can be a full, illustrated, written report to facilitate settlement or in the form of a declaration under oath.
- Question formulation to examine opponent's document examiner; also pertinent citations for cross-examination from learned treatises.
- Identification of the writer of poison pen letters and anonymous writings.
- Other aspects of questioned document examination.
- Impeachment of erroneous, incomplete or incompetent expert handwriting opinion.
- Consultant to other document examiners.

CREDENTIALS:

- Certification with commendation, Paul de Sainte Colombe Center, January 28, 1981, with honorary Charter Membership Certificate, April 8, 1982.
- Consultant and mentor to other document examiners.
- Honorary membership, National Questioned Document Association, March 1992.
- Consulted by several public agencies on administrative issues.
- San Francisco Tax Registration Certificate. Expiration date: 12-31-98.
- Consulted by various governmental agencies.
- In *Jones vs. Hester,* Denton County, TX, under *DuPont/Daubert* guidelines Judge Donald R. Windle ruled Mr. Matley "meets the standards for scientific endeavor."

EXPERT WITNESS:

- Accepted in Federal Bankruptcy Court and in Superior and Municipal Courts, and Administrative Law Courts, California; Probate Court, Texas.
- Testimony in administrative and arbitration hearings and in depositions.
- Complete listing of testimonies available upon request.

CLASSES AND LECTURES:

- Private classes have been offered since 1981.
- Lecture series titled: Seminars and Workshops in Handwriting Sciences, for professionals and advanced students.
- Organizer, co-producer and instructor at the annual Seminar in Document Examination, in San Francisco. Attendees from U.S. and Canada.
- Presenter at NADE Conferences and Pre-conferences: 1992: *Human Graphic Motor Sequence.* 1993, two-day intensive course: *The Physiology and Forensic Identification of Handwriting.* 1994: *Answering Challenges on Voir Dire.*
- Presented workshop, *Qualifications Impeachment*, ABFHA 1993 Conference.
- 1995 IGS-AFDE Joint Conference: *A Protocol for Research in Disguised Handwriting as Illustrated by Slant.*
- 1995 AHAF Symposium: *The Physiology of Handwriting, a Half-day Workshop.*
- 1997 Vanguard Conference: *Physiology of Handwriting as Exemplified by Writer's Cramp.*
- 1998 Vanguard Pre-Conference: *Speed in Handwriting.* An all-day intensive.

LISTINGS:

- *FORENSIC REGISTER OF EXPERT CONSULTANTS,* Bar Association of San Francisco.
- *Who's Who in Finance and Industry, 30th Edition,* Marquis Who's Who, Publishers.
- And others.

PUBLICATIONS:

- Forgery: Detection and Defense; a Guidebook for the Legal Professional, 1988.
- QDE Index: Periodical Articles in English on Document Examination, Handwriting Expertise and Expert Testimony. 1997 edition published by Shunderson Communications, Ottawa, Ontario, Canada.
- *Oh, My aching arm: The cause and cure of general writer's cramp, 1997.*
- STUDIES IN QUESTIONED DOCUMENTS. 1. "Photocopies in Document Examination." 2. "Exorcism of Ignorance; a Reply From a Handwriting Expert." 3. "Logged Entries: Made Separately or Sequentially?" 4. "Exemplars for Comparison." 5. "Health and Handwriting, an Annotated Bibliography." 6. "The Employing and Examining of the Expert." 7. "Reliability Testing of Expert Handwriting Opinions." 8. "Signature Disguise by Change of Slant."

PUBLICATIONS, CONTINUED:

- Also published in: AHAF Journal; Journal of the National Association of Document Examiners; Communique, The Document Examiner's Newsletter; California Paralegal Magazine; At Issue (San Francisco Association of Legal Assistants); Write Extension; AHAA Dialogue; Journal of Forensic Identification; Vanguard; The National Document Examiner; San Francisco Attorney; Scientific Sleuthing Review; The Recorder (San Francisco); International Journal of Forensic Document Examiners.
- Other writings in the field, published and unpublished. The published books and monographs have been marketed to professionals nationally and internationally.
- Published works in other fields.

EDUCATION:

- Saint Francis College, El Cajon, CA, 1952-54. Liberal arts, science.
- Immaculate Heart Seminary, University of San Diego, San Diego, CA, 1954-60. Courses included Philosophy, Logic, Rational Psychology, History, Rhetoric.
- Immaculate Heart College, Los Angeles, CA. MALS, June 1963.
- San Diego State College, San Diego, CA, 1962-63. Graduate studies, Political Science.
- 1979-80 privately instructed by Rose Toomey, author of HEALTH CLUES IN HANDWRITING.
- Seminar on the handwriting of criminals, conducted by Ted Widmer of San Francisco.
- Third Annual Conference for Testifying & Consulting Experts, San Francisco, 1996. Sponsored by Council for the Advancement of Science in Law. Also pre-conference symposium: *Preparing for Cross-Examination: Developing 'Active Listening' Skills.*
- Professional Education Systems, Inc., *California Evidence Workshop,* San Francisco, CA, December 21, 1996.
- Oregon Criminal Defense Lawyers Association, Winter Conference, *Expert Witnesses,* December 5-6, 1997, Portland, OR.
- American Academy of Forensic Sciences, Conference, February 8-14, 1998, San Francisco, CA. Attended entire Questioned Document Section, and Jurisprudence Section: *Erroneous but Convincing.*
- Oregon Criminal Defense Lawyers Association, Winter Conference, *Dynamic Cross-Examination,* James H. McComas, December 3-4, 1999, Portland, OR.
- On-going research in the legal, forensic and academic literature.
- Primary research in methods of disguise, writing with the opposite hand, and others.
- Attendance at conferences at which I have presented. See above under "CLASSES AND LECTURES."

SPECIAL RESOURCES:

- Personal collection of more than 6,000 books, monographs and papers in handwriting science, document examination and case law, along with current subscriptions.
- Developed proprietary, computerized databases of approximately 10,500 citations to forensic, academic and legal periodicals and to case reporters. This is an on-going project.
- Memberships in NADE and AHAF bring access to extensive lending libraries of professional literature.
- Microphotography and optical filters.
- Optics such as stereoscopic microscope and other standard tools.
- Specialized equipment such as Kinderprint Indentation Materializer (equivalent of electrostatic detection apparatus), LEE Tek IVS-100 (video IR and UV light document analyzer), various filters.
- Association with professionals having specialized skills and equipment so that unique or unusual requests can be referred to highly qualified colleagues.

MEMBERSHIPS:

- National Association of Document Examiners (NADE). Chairman of 1993 Conference in San Francisco.
- American Handwriting Analysis Foundation (AHAF), an international organization based in San Jose, CA. Foundation Librarian and member of the Board.
- Borrowing member of American Association of Handwriting Analysts Research Library, Downers Grove, IL.
- International Graphonomics Society, Society for the Science and Technology of Handwriting, headquartered at University of Nijmegen, The Netherlands.
- Special Member, Vidocq Society, Philadelphia, PA.
- The American Society for Testing and Materials. Member, Committee E30 on Forensic Sciences.

REFERENCES

Available upon request.

(01-19-00)

Kimberly A

eagerly

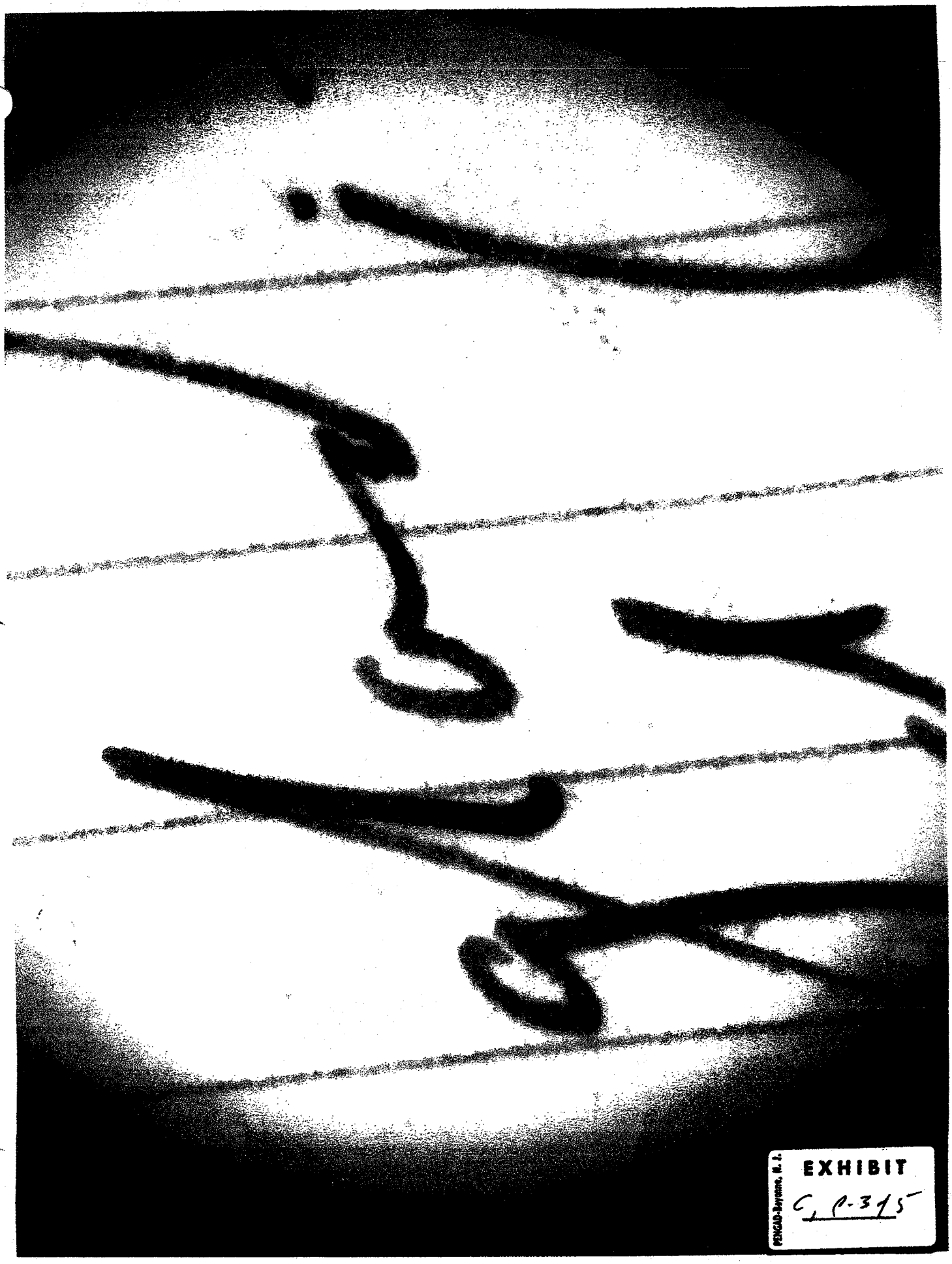
EXHIBIT
C, P-315

PENCO-Bayonne, N. J.

EXHIBIT

C.P. 415


EXHIBIT
GP-515
PERKINS-Bayonne, N. J.

EXHIBIT D

PENGAD-Bayonne, N.J.

Av —

I am to set [illegible] — [illegible]

from [illegible] and from

[illegible]

[illegible]

[illegible] came here

to explain why I was there in picture.

with the [illegible] and others

in Calif. [illegible] — this should

provide help against [illegible] claims

[illegible]

I, Marcel B. Matley, swear and affirm:

1. I am an Examiner of Questioned Documents and have been so employed since 1985. My business office is located at 3092 Army St., San Francisco, CA. I have been certified as an expert witness in the field of questioned documents in Federal Bankruptcy Court, California Municipal and Superior Courts, in Arbitration and Administrative Hearings in the State of California, and in Probate Court in the State of Texas, and I have testified regarding documents in each of these areas. A copy of my current Curriculum Vitae and General Resume, outlining my training, experience, qualifications and professional activities, is attached hereto as Exhibit A and incorporated herein as if set forth in full.

2. I was asked to evaluate the transcript of the testimony of Gus Robert Lesnevich (hereinafter: "Lesnevich") given in the case *United States of America vs. Lawrence X. Cusack, Jr.*, United States District Court, Southern District of New York, Case No. 98 Cr. 691 (DLC) (hereinafter: "*Cusack Case*"). I was asked to evaluate it in terms of its validity and reliability in light of the standard and accepted theory and methods in the field of questioned documents. In summary, my evaluations are:

(a) If Lesnevich's theoretical statements about how handwriting can be identified were correct, it would be impossible to identify handwriting.

(b) If Lesnevich's theoretical statements about how handwriting can be identified were correct, then Lesnevich

did not offer a scientifically or technically reliable opinion, because he did not apply the theory which he said underlies handwriting identification.

(c) If Duayne J. Dillon (hereinafter: "Dillon"), the other Government handwriting expert in the *Cusack Case*, and Lesnevich were correct in their evaluations of the questioned writings, the writings cannot be either identified as to who made them nor proven not to have been made by John F. Kennedy (hereinafter: "JFK").

(d) Lesnevich showed lack of familiarity with relevant scientific publications which contradict some of his assertions.

(e) Lesnevich never proved any necessary nexus in his opinion about the cause of the questioned writings, but he merely asserted such nexus.

(f) Finally, Lesnevich employed a methodology which can be used to prove either of two contraries and he asserted contrary "facts" depending on which prong (writings are forged or Defendant forged them) of the prosecutor's case he happened to be supporting at the moment.

3. At 1816:2-4 Lesnevich was asked: "Can you explain how it is possible for you to determine through forensic analysis who wrote a particular item of questioned handwriting?" The answer covered 1816:5-1818:2 and stated that the following, among other factors, make a handwriting identifiable:

(a) The questioned writing "must be someone's normal natural writing." At 1816:8-9.

(b) The exemplars "must also be someone's normal and natural writing." At 1816:9-10.

(c) The writing being examined must differ from the copybook the writer first learned in school. At 1816:11-13. Presumably that goes for both the questioned writing and exemplars since it was only stated that "the writing that you are going to examine" must be such.

(d) The writer has developed "individual habits of writing." At 1817:18-23.

(e) The person writes "rapidly and quickly." At 1817:23 *et seq.*

4. Based on those five requirements, the Government's burden of proving either forgery or forgery by Defendant could never be met, nor could Defendant or anyone else even know whether the writings were false. This is shown by addressing each requirement in turn:

(a) Since the questioned writing "must be someone's normal natural writing," the questioned writings in this case are impossible to identify, since they are, according to both Lesnevich and Dillon, forgeries by imitation and/or tracing. Both of these experts explain all the allegedly unnatural and non-normal features of the questioned writings which supposedly prove them to be forged. Therefore, identification of the writer is impossible, which also means proving who did not write them is impossible. Why the latter impossibility? If no one can be identified as the writer of the questioned material, neither can JFK be identified as the writer. If JFK cannot be identified as

the writer, to prove he did not write them one would have to prove that it is impossible for him to write in such an unnatural and non-normal way as the questioned writings are supposedly written. To do that, one would have to have samples showing how JFK would make unnatural and non-normal writings. But again, if we had JFK's unnatural and non-normal exemplars, we could not use them to identify him as having written the questioned materials, since we are back up against the thesis that writing has to be natural and normal to be identified. So we can never determine whether or not JFK wrote in what the Government experts claim to be the unnatural and non-normal way. Therefore, according to the theoretical and factual bases asserted by Dillon and Lesnevich, no one can either prove or disprove that JFK, *nor anyone else*, either wrote or did not write the questioned writings. Defense counsel never challenged this self-created, theoretical and practical impossibility for the Government to prove its case.

(b) The exemplars "must also be someone's normal and natural writing." But if the exemplars are in a normal and natural writing, they could never be validly compared to questioned writings which are allegedly unnatural and non-normal, being purported imitations and/or tracings.

(c) If the writing being examined must differ from the copybook which the writer first learned in school, the expert must prove what copybook the writer was taught, and also what precise differences exist between it and the writer's exemplars *at the time period* comparable to the

questioned writings. Then the expert must prove that the questioned writings are also identifiable. To do that, he must prove that they differ from the copybook which the writer of the questioned writings was first taught. But to do that, he must know who wrote them, which cannot be known until he proves the questioned writings are identifiable, which is a logical vicious circle. Defense counsel never attacked this theoretical and practical impossibility arising from Lesnevich's theory and reasoning.

(d) The writer must have developed individual habits of writing. Therefore, before making an identification, the expert must prove what precisely are the individual writing habits of the suspect *and* of the unknown writer of the questioned materials. He must then prove what graphic traits are directly attributable to those writing habits. Then he must prove that those graphic traits *in combination* belong to one and only one potential writer, or at least to only one of the entire field of potential writers of the questioned materials. For JFK material, that would at least be the Defendant, JFK, and all secretaries who could have possibly written something for JFK at his request. Persons intimately familiar with the *Cusack Case* might be able to name other reasonable suspects who would also have to be considered. Lesnevich never did any of that during his testimony, nor did defense attorney require him to.

(e) The person must write "rapidly and quickly." However, it is the opinion of the Government handwriting experts in the *Cusack Case* that the questioned writings,

being imitations and/or tracings, are *ipso facto* written slowly, not rapidly and quickly. Therefore, one could neither identify who wrote them nor prove that JFK did not trace or imitate himself, *unless* one were to prove that it was physically impossible for JFK to either write slowly or imitate himself or trace his own handwriting. So once more, Lesnevich's theory logically makes it impossible for the Government to prove either authorship or non-authorship of the questioned writings. Defense counsel did not challenge this point either.

5. Lesnevich makes scientifically incorrect statements, that is, he asserts things which published research shows to be wrong. For example, at 1817:5-10 he says children are shown by the teacher how to hold the pen. "When the teacher leaves, the student is going to move that pencil in their hand a little bit to what is comfortable for them." As a matter of fact, such published research as "Qualitative changes in dynamic tripod grip between seven and 14 years of age," by Jenny Ziviani, *25 Developmental Medicine and Child Neurology*, Dec. 1983, pages 778-782, demonstrates that even with the tripod grip (i.e., using thumb, forefinger and middle finger only to hold the pen) children tend at first to grip far too tightly, something that is anything but comfortable, while some adopt variations impeding ease of movement. Lesnevich made other statements which are contrary to findings in relevant scientific publications. Defense counsel never challenged Lesnevich to support such statements with learned treatises.

6. At 1819:7-9, Lesnevich said: "The questioned text was naturally and spontaneously written. It is someone's everyday writing." That contradicts what Dillon said of it, that it was an imitation and/or tracing, not a natural and normal writing, but one which was made deliberately and slowly. However, Lesnevich also contradicts himself. At 1856:23-1857:16 he explains how the questioned writing is "drawn" in contrast to genuine writing which "is done spontaneously and rapidly and quickly." Depending on which prosecution point he needs to insinuate (authorship of the questioned material or falsity of the questioned material), Lesnevich adopts a different evaluation of the questioned material. Defense counsel did not challenge any contradictions of Lesnevich made by himself or Dillon.

7. Lesnevich did not follow standard, established methods for a forensic handwriting comparison. One instance will serve to illustrate. At 1842:8-11, he said: "The chart depicts certain writing features, *certain letters* found in Government Exhibit 503 and the comparison of *those individual writing characteristics* to *some* of the known handwriting." [Emphases added.] Several violations of proper methodology are contained in that one sentence, among which are:

(a) Using "certain letters" means the letters are isolated from their context. This is a method by which an expert for hire can "demonstrate" that any given author either did or did not write any given questioned writing. As long as one has enough sample writings which are in the

same language within the same general educational or national group, there will be some isolated same letters from two groups of writings which will be in some way "the same" and other isolated same letters which will be in some way "different."

(b) "Individual writing characteristics" are not what identify handwriting. Rather, it is those traits which are characteristic of the handwriting as a whole. In other words, an identifying characteristic is one which would appear throughout all or most of the writer's authentic writings and which is individually unique within the context of the entire writing in which it appears. Handwriting experts speak of "habits," which is a happy term, for it requires that an identifying trait be done by second nature, that is through an acquired disposition, that it be done more often than not when occasion for it being done arises, and that it be difficult to act in a contrary manner. Further, identifying writing characteristics cannot identify in an individual way, that is, no characteristic by itself can be taken as sufficient evidence of identity. It must combine with other identifying characteristics in a composite which reasonably eliminates all other writers than the one under consideration. In Lesnevich's method, *any* suspect could be proven to have written all undisputedly genuine JFK materials as well as be proven not to have written all of *his own* genuine materials. The hunt-and-pick method of isolated letters and "individual" characteristics permits such performance.

(c) Lesnevich said he compared the questioned materials to "some" of Defendant's exemplars. The identification must hold up in any comparison made with *any* valid exemplar by Defendant, otherwise Defendant did not write that particular exemplar, albeit concededly genuine. That little tell-tale word "some" shows a biased selectivity in the comparison process, a bias and selectivity which defense counsel did not explore.

8. Standard texts leave no doubt that Lesnevich did not apply correct theory and follow proper procedure. For example, Albert S. Osborn, in his book *Questioned Documents, Second Edition*, reprinted by Nelson-Hall from the 1929 publication by Boyd Printing Co., at page 380 said: "As we have already seen, writings which follow the same general style must inevitably show certain similarities (Figure 205) and, if similarities alone are searched for, as is often the case, an erroneous conclusion is not only possible but probable.

"If error is to be avoided, a basic fact that must constantly be kept in mind in an investigation of the authorship of a writing is, that handwritings by different writers in the same language, and especially those following the same system of writing, are bound to resemble each other in certain ways. As in many other phases of the investigation of documents, it therefore becomes necessary to analyze the resemblances and the differences and give them their proper weight. The incompetent and inexperienced examiner is, of course, not qualified to do this and his

ignorant findings should never be blindly followed. The incompetent accuser is sometimes effectively silenced by being required to write the disputed matter from dictation, when it at once appears that his writing in numerous ways is similar to the disputed writing, as any writing in the same language is bound to be."

Figure 305 on page 379 shows 14 instances of "and" by 14 different writers. Applying the methodology Lesnevich used in the *Cusack Case*, the same person wrote all 14 of them.

9. At 1843:24-1844:2 Lesnevich talks about "writing movement" in letters "A" which have been isolated from the questioned and exemplar materials. That shows either lack of perception of what "writing movement" means or a clever misapplication of the term. To explain it by analogy, it is like speaking of Michael Jordan's basketball movement as shown in a still picture of him on the court. His movement would only be seen in a "moving" picture. Thus the picture of an isolated letter or the discussion of an isolated stroke is a still picture of handwriting, while an entire word or passage alone provides a "moving" picture of handwriting movement, *if* the expert explains the pen's dynamics as it traced the entire word or passage *and* the writer's physiological dynamics as he moved the pen in tracing the entire word or passage. Later at 1847:13-23 Lesnevich explains how he used arrows to show identifying traits. Arrows point, and they point to a point, a static, specific spot, and thus they are totally devoid of ability to designate movement, and it is the writer's movement while

writing which is uniquely individual and causes uniquely identifying effects in the writing. Unless those effects are specifically and accurately tied to their causative movement, they cannot be tied to the writer who did the moving. Defense counsel did not require that tie to cause.

10. Lesnevich often stated whether or not "we" were taught something in penmanship class. There was neither objection by defense counsel for such speculative opinion nor challenge to document it. Since the premise of Lesnevich's opinions in the *Cusack Case* should have been what Defendant, JFK or other writers under consideration were taught in penmanship class, not "we," Lesnevich's conclusion could only hold up if those writers' specific educational experiences are proven. However, no proof of such was ever forthcoming, merely assumed on the basis of the speculative assertion of what "we" were taught. Defense counsel did not challenge Lesnevich to prove such premises which were essential to his opinion.

11. At 1847:1 Lesnevich mentions "similar handwriting habits" as being a basis of his opinion. Defense counsel should have challenged him to meet the requirements which statute and case law set forth for proof of habit. Not being an attorney, I could not say what those would be, but certainly an expert ought not be allowed to escape without satisfying requirements of the law for the type of testimony offered. I can say that the testimony did not satisfy the requirements of forensic handwriting expertise for demonstrating handwriting habits, a notion I have touched on

already.

12. At various places, such as 1854:6 *et seq.*, Lesnevich speaks of fast and slow writing. Defense counsel never challenged him on *how* he can determine speed in writing and what research supports his way of doing so, nor tested him to determine speed in test writings, the production of which was independently and objectively timed. That would force the expert to say whether he discerns absolute or relative speed, whether he determines overall speed or relatively fast and slow portions. Such penetrating cross-examination is the only way to expose fatuous assertions as, at 1860:4-10, that Defendant's range of variation showed ability to forge:

(a) First, one must prove the cause of the variation, whether physical impairment, different types of documents, defective writing materials or tools, versatility as a penman, etc. Lesnevich never did that nor did defense counsel challenge him to.

(b) Variance in one's own writing and ability to imitate another are entirely two different dynamics. The latter requires ability to perceive alien forms, ability to change one's own personal tendencies to variation into another's tendencies to variation, and so on. The former does not require these things. Lesnevich should have been challenged to prove every purported nexus between these two unconnected dynamics.

(c) There is absolutely no support in the literature of forensic handwriting identification to support such a

notion, nor did defense counsel demand that Lesnevich document such a fallacious thesis.

13. At 1856:23-24 Lesnevich said: "Natural writing is something you do without any thought." Defense counsel could have argued to the jury: "Are you really silly enough not to think about what you are writing and how you are writing it? Imagine what your check book would be like if it were something you do without any thought?" And so on. Lesnevich then said that "you" write your name very quickly and pay no attention to it. However, many people write their name slowly and carefully, and some write at different speeds at different times. According to Lesnevich, if "you" slow up "your" signature (whoever "you" might be), "you" did not write it! He either forgot or did not know that what must be proved *prior* to that kind of argument from speed of execution is one of three things:

(a) *this* writer *never* wrote his name slowly; or

(b) *this* writer *only* wrote his name slowly in these proven circumstances; or

(c) *this* person writes slow signatures in this manner and fast signatures in another manner, both manners being demonstrated by genuine signatures.

14. In other words, generalized statements such as Lesnevich made about an unnamed, amorphous "you" are totally speculative and without foundation in any supporting research. Therefore, they cannot be premises to prove what a specific person did or did not do in a specific situation. It is expert proof by avoiding both expertise and proof.

Expertise must be rooted in sound, documented theory, and proof must be rooted in specific evidence of specific facts about a specific writer regarding a specific writing through a specific combination of specific, demonstrated and uniquely individualizing characteristics. An *a priori* presumption of underlying data versus objectively documented data is the hallmark of the pseudo-expert. Lesnevich did not fulfill any of these standards, nor did defense counsel expose the emptiness.

15. Lesnevich referred repeatedly to Defendant's writing abilities as proof of his having made the alleged forgeries. Besides the fact that no amount of ability is proof that anyone ever did a specific act, there are three specific kinds of skill in handwriting, each *per se* non-inclusive of either of the other two:

(a) Skill in writing the way teacher says to write;

(b) The skill which is known as "graphic maturity," whereby one writes in a personally unique and spontaneous manner for a sentence or more with an uninterrupted graphic impulse; and

(c) The skill to imitate another's writing. Lesnevich falsely assumes that possession of either of the other two types of skill necessitates possession of this third, a most ill-advised and presumptuous assumption.

16. At 1866:11-15 Lesnevich assumes as true an even more fallacious theory about skill, that drawing ability equates to ability to imitate another's handwriting. His logic apparently is:

(a) Major premise: Forgery is a drawn writing.

(b) Minor premise: Doodles are drawings.

(c) Conclusion: Therefore ability to doodle is ability to forge.

17. However, handwriting and the art of drawing are separate skills within separate disciplines, otherwise Ludovico degli Arrighi, the greatest Renaissance writing master, was the greatest Renaissance artist, greater than da Vinci or Michelangelo. Research in this relationship shows a few common factors but no necessary correlation. Jan Melou in his paper, "Does drawing skill show in handwriting?", *3 Character and Personality*, March 1935, pages 194-213, surveyed the handwriting and drawing ability of 250 school children. At 212 he stated: "[D]rawing skill does not necessarily promote the quality of penmanship; for many good draughtsmen have a very clumsy handwriting; and vice versa, many calligraphists are poor draughtsmen. Pure calligraphy, unaccompanied by other symptoms of draughtsmanship, is hardly ever an indication of drawing skill." Further on he stated: "Both drawing ability and writing skill are complex faculties, each depending on a number of factors." And at 213: "Some of their specific factors appear to be antagonistic to each other." Later research gives the same indications: some relationship between drawing and handwriting but significantly diverse factors. Thus see also Fracesco Lacquaniti, *et al.*, "The law relating to the kinematic and figural aspects of drawing movements," *54 Acta Psychologica*, 1983, pages 115-30. In

summary, Lesnevich made a most unscientific and fallacious argument regarding doodling as *a priori* evidence of skill at imitating another's handwriting. Nor did defense counsel demand supporting authorities for such a thesis, whose sole recommendation is its convenience in supporting the client's legal theory.

18. At 1870-1871 Lesnevich reduced his proof that Defendant wrote the alleged forgeries to a mere showing that he could have. A chart was offered to demonstrate such purported expert proof of guilt. Defense counsel objected neither to the false premise that mere possibility is evidence of actuality nor to the admissibility of a chart to support such fallacy. Further, if all the testimony Lesnevich gave that Defendant wrote the alleged forgeries did not at all prove that he did, only indicated he could have, then all that same testimony never proved that JFK did not write the questioned JFK material, since JFK equally had the ability to imitate himself, and even vastly more so than Defendant had, as did the several secretaries who in fact did imitate JFK at his request. Thus, speaking technically and scientifically, no proof of falsity was made even to a reasonable probability, much less beyond reasonable doubt. Indeed, at 1883:18-1884:2 Lesnevich stated what he would have had to do to prove JFK did not in fact write the questioned material, thereby admitting he never proved falsity, merely accommodated the prosecution's thesis of guilt. Yet defense counsel made no challenge to this travesty of an expert handwriting comparison.

19. One could write an encyclopedia on all the erroneous theories, reasoning and data in the Lesnevich testimony. To address fully the many times when defense counsel validated Lesnevich's fallacies during cross-examination would take a book. However, a few telling facts emerged during cross-examination, but defense counsel never exploited them. For instance:

(a) At 1910:16-1911:14 Lesnevich is made to admit he never compared Cusack, Sr.'s handwriting to the questioned material. If they are not by JFK, every reasonable maker of them ought to have been considered. It seems that Lesnevich only did what the prosecutor wanted him to do: insinuate as well as possible that they were forged and that Defendant forged them.

(b) Lesnevich evidenced no keeping of a record of activities or even of his charges and income in the case. Answers to such enquiries were expressed with such terms as "I believe...," "I don't believe...," and "I don't recall...."

(c) At 1916:7-17 Lesnevich is made to admit he could never tell whether or not Defendant was writing deliberately or "it just came flowing subconsciously." However, based on his earlier thesis of spontaneous, fast, quick writing being genuine versus slowly drawn writing being false, he could never tell whether Defendant's genuine writings are genuine or not and thus identifiable or not, nor whether they are valid comparison material or not. Equally, the thesis of Defendant's ability to forge is unfounded since we can no

longer know whether or not his writing is drawn and thus whether or not he can "draw" handwriting, a skill required for forgery according to Lesnevich.

(d) At 1919:18-25 Lesnevich is made to admit that he cannot tell whether Defendant changes his writing subconsciously or knowingly. If he cannot tell that, he has no basis to assert that Defendant can deliberately imitate JFK, which is to change his writing knowingly versus subconsciously. If the expert cannot know what he himself asserts to be an essential element in forgery, there can be no expert evidence of forgery.

20. At 1921:13-19 questions and answers establish that Lesnevich was hired to determine Defendant's artistic ability. Artistic ability is pertinent to an entirely different field of expertise than handwriting ability is. Defense counsel did not object to such delving into an expertise the witness had not been qualified in.

21. There is not a single aspect of Lesnevich's testimony which cannot be shown to be either an incomplete or inaccurate statement of forensic handwriting identification theory or method. The above must serve as illustrative of what could be established in contradiction of his testimony, if one labored at it for more than a week. It all amounts to the echoing of the client's claims about the handwriting in question, supported solely through expert opinion by innuendo, supposition and speculation. It is 134 pages of testimony designed to substitute plausible insinuation in place of compelling proof of forgery.

I, Marcel B. Matley, swear and affirm under penalty of perjury under the applicable laws of United States of America that the foregoing is true and correct to the best of my knowledge and belief. This affirmation was executed on this _____ Day of _____, 2000, at San Francisco, California.

_____

Marcel B. Matley